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LECTURES

ON

DISEASES OF THE BRAIN AND NERVOUS SYSTEM,

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LECTURE I.

PRINCIPLES OF INVESTIGATION.

GENTLEMEN,—In the course which I am about to deliver this session, I propose to draw your attention specifically to diseases of the brain and its appendages; in a word, to affections of what is called "the nervous system,"—those diseases which affect the organs of relation, commencing with the nervous system as the chief agent by which the various acts or functions of relation are produced and regulated.

Before we enter immediately upon the study of this most interesting class of diseases, permit me to present to you a few preliminary considerations on the difficulties by which the physician is surrounded in his pursuit of knowledge in this particular branch of medicine. The study of diseases of the nervous system is peculiarly difficult, not only on account of the obstacles which impede the advancement of medicine in general, but from various circumstances connected more intimately with the system itself. In the first place, if we desire to collect materials for the ground-work of any general views upon this subject, we find ourselves compelled to examine a vast variety of works, and to spend an immense portion of time in the collection of facts, which are scattered through a great number of authors, observed at different times, and published in different tongues. A great deal has been accomplished, particularly in latter years, towards increasing our knowledge of the functions of the nervous system, and its pathological anatomy; but I know of no single work which may be said to afford any thing like a complete view of the question, which embraces all the facts that bear upon it, and at the same time does not leave untouched a great number of the most important and interesting points on which correct principles are to be founded. But the subject, difficult and arduous as it is, from its own nature, is rendered still more difficult from the various opinions and theories which prevail amongst the best physicians on almost every single question connected with the physiology and pathology of the brain and nervous system. It is not now the time to touch upon the disputes to which these have given birth, but you will have occasion to observe, during the present course, how very difficult it is to arrive at any thing like the truth, or even to obtain a fixed idea on any point, amidst the multitude of conflicting opinions, and the host of contradictory facts, which we shall find stated and vouched for upon the most respectable authorities. Yet all these we must examine and discuss, with the hope of obtaining some determinate principles, carefully sifting opinions, and weighing and comparing the facts on which they have been founded. I do not mean to affirm that medicine, as a science, is exact or complete. No; I certainly would not venture to assert *that*. There are many points in the science upon which we are as yet by no means sure; but uncertain as our knowledge may be with regard to the causes and nature of several diseases, it is infinitely less sure, far less firmly based upon rational grounds, or supported by positive *data*, when the diseases of the nervous system become the object of our inquiry. In the study of any disease, or in the investigation of the diseases of any particular part of the body, or any system, there are three or four circumstances to which the attention of the physician is almost exclusively directed, and which he always endeavours to determine, though he may not expect that his efforts will always be crowned with success. His *first* object is to ascertain the causes which have determined the disease: his *second* is to observe the external or internal symptoms which reveal its existence: his *third* is to study the pathological phenomena or lesions

which the disease produces in the different tissues; and, *finally*, by carefully meditating on the causes, the symptoms, and the pathology of the disease, he endeavours to fix upon a rational treatment. This is the manner in which we should proceed, were we to investigate diseases of the chest, of the abdomen, or of the circulating or any other system; but in diseases of the nervous system, we unfortunately find that the study of the causes, the symptoms, and the treatment, is surrounded with difficulties of a peculiar nature, which we do not find in any other class of disease. We shall now lay before you an account in detail of some of those difficulties, – not with a view to discourage you in the pursuit of professional knowledge, but in order to render your progress more easy, by pointing out at once some of the principal obstacles with which you will have to grapple, and prepare you to surmount or remove them.

If we begin by investigating the causes which determine the symptoms of other diseases, we have to consider *first*, the action of external agents; *secondly*, the reaction occasioned by the operation of several of those agents; and *thirdly*, the process or influence which is called "sympathy," — a trouble produced in the functions, and often in the structure, of one part of the body, or of the economy in general, by derangement in the function or structure of another part.

Applying this division to the nervous system, we find that the latter is not influenced by the external world, or at least, is but little subjected to those changes which external agents incessantly tend to produce in other symptoms. The respiratory apparatus is constantly and of necessity exposed to the action of the atmospheric air, and to the various impressions, of a thousand sorts, which are apt to be conveyed through that medium. The intestinal canal is also daily exposed to the action of the various alimentary and other substances which are applied to its surfaces. But the central nervous system is, by a peculiar provision, withdrawn from the influence of most agents of this kind. It is, however, subject, in the the highest degree, to effects of the third species of causes to which we have alluded. Indeed, an attentive observer of the phenomena which take place during disease, must be convinced that we can have no organ slightly deranged, no function troubled, even in an insignificant manner, without a corresponding change being produced in the nervous system. No matter whether the primary change takes place in a fluid or in a solid of the body, in an organ essential to existence, or in an apparatus of secondary importance. The central nervous system, or that force which presides over life, partakes more or less of the disturbance. Its ordinary phenomena are then no longer manifested, and the economy evinces symptoms of general trouble. Hence, in all diseases, the nervous system is affected in a variety of ways; and hence, also, arises one great difficulty in pursuing the study of those diseases which are peculiar to it. Take, for example, *any* disease, examine its symptoms, separate those which depend immediately upon the organic change from those connected with a lesion of innervation, — select a number of diseases indiscriminately, and see how many of the most important among them frequently depend upon the latter. In gastritis, in hepatitis, in inflammation of the lungs or the abdominal viscera, how many symptoms have we which can be referred to the mere inflammation of the part,—to the simple physiological change which occurs in the condition of its vessels? Examine the symptoms with care, and you will find that the greater part of them depend upon the trouble occasioned by the local disease, in altering or perverting the vital force,—that they are, in a word, secondary affections of the nervous system, which commands and directs all the other systems of the economy,—which stamps its peculiar physiognomy on the primary disease, determines its character and gravity, regulates its march, produces its complications, and, finally, directs the therapeutic indications which we are to follow. In almost all the other organs of the body, we can find the cause of the malady in the structure of the organ. Pathological anatomy, cultivated as it has latterly been with ardour and perseverance, has laid open to us the changes which take place in the organic structure of the different apparatuses during the course of most diseases by which they are affected; but in the nervous system pathology furnishes little or no aid of this kind, and we are compelled to look for a number of causes of its diseases which are totally unconnected with physical modifications. Hence arises a principal difficulty in the study of nervous diseases of the system, which assume a variety of characters and types, under the influences of causes which, having no sensible effect that is appreciable to the senses, often escape our observation, or involve us in an obscurity which it is too often impossible to dissipate. Though our knowledge of nervous diseases may still be very imperfect, we are sufficiently advanced to affirm that they vary considerably, according to the different stages and periods of life; that they are modified by climate, and even by atmospheric changes; that the tendency to several nervous affections is augmented or influenced by the education and profession of the individual, and by numerous other moral agents, as must be familiar to every physician who has practised medicine successively in the country and in large populous capitals, like Paris. Finally, according to the form of government, the religious persuasion of the inhabitants of a

state, according to the degree of civilization, and the more or less extensive partition of enjoyments which operate with immediate influence on the moral man, we find that diseases of the nervous system become at one time intense, at another feeble, are confined to particular classes, or are diffused through vast masses of the population. Diseases which at one time were obstinate of treatment and frequent in recurrence, become at another time mild and rare. Affections with which we are now familiar, were seldom observed in earlier and more simple ages; while many nervous diseases have altogether disappeared and become extinct, from the different relations of man in civilized life.

This uncertainty in the form and character of disease, more peculiar to nervous disorders than to any others, from some of the causes we have just pointed out, must render it difficult to appreciate a great number of circumstances connected with them; but this is not all: even when we would determine the existence of any nervous disease, seek its place in the nosological scale, in a word, give a name and seat to the disorder before our eyes, we find ourselves surrounded by a number of peculiar difficulties which frequently frustrate our best-directed and most assiduous efforts. Some, perhaps the principal, of these obstacles, gentlemen, arise from the absolute insufficiency of our means of investigating diseases of the brain and nervous system. Disease of an organ is betrayed by a change in its physiological functions. The change of structure or of physical conditions in many of the chief organic apparatuses, makes itself known to our senses, and hence arises the admirable precision of diagnosis at which we have latterly been enabled to arrive in diseases of the chest and abdomen. By the aid of auscultation and percussion, or, in other words, by bringing our senses to bear immediately upon the injured part, we can fix with wonderful accuracy, not only the seat, but even the precise nature, of various disorders which occur in those two great cavities; but the peculiar nature of the functions of the brain and nervous system, the difficulty hitherto experienced of establishing any connection between the function of the organ and its structure, the absolute impossibility of applying our senses with any advantage to an appreciation of the normal or abnormal physiological conditions,—these and several other causes compel us, in diseases of the nervous system, to abandon the aid of our senses, and have recourse to simple induction. But this latter guide is uncertain: it furnishes to different investigators, an immense difference of results, and thus the study of nervous diseases, difficult as they are on account of their nature, becomes still more difficult from the insufficiency of our means to pursue it.

A second obstacle which presents itself to us in our examination of diseases of the brain and nerves, arises from the absurd manner in which theory has been mixed up with observation. The number of facts collected is considerable, but their value has been much diminished, and their appreciation often rendered obscure, if not impracticable, from the unhappy tendency which too many writers have, to build up suppositions without any foundation, which they bring forward at every instant, and to which the true facts are frequently compelled to yield, or they are distorted into an apparent reconciliation. Thus for many years the study of diseases in general, and of the nervous system more particularly, has been retarded by a wish to explain all that we witness in the course of disease, by ascribing it to an augmentation or a diminution of normal excitement. No doubt in many disorders which have an hyperdynamie, or an adynamie, we find the vital force elevated beyond the normal type, or depressed below its natural standard; but in a great number of diseases we have a third state, which we cannot neglect without falling into numerous errors; we have something more than a *plus* or a *minus*; we have a condition that cannot be referred to exaltation or diminution of the vital force; we have a change of quality in that force, an aberration, a perversion or deprivation whose influence is perceptible in almost every affection, and whose existence we cannot neglect without exposing ourselves to all the errors of the Brunonian doctrine. The existence of this state of ataxia in various disorders may be directly proved, in others it may be inferred from analogy; thus in delirium, in several forms of convulsions, &c., we have evidently an aberration or a deprivation of the vital force; not an exaltation of it, or an excitement. When delirium exists in the course of fever, or of inflammation of the brain, I say there is an aberration of the intellect, a deprivation of the vital force in that part of the economy, and I can prove this from the very nature of the disordered function, but I cannot admit that there is an excitement or an elevation of a normal condition.

Another cause of the difficulties with which we meet in studying diseases of the nervous system, arises from the circumstance that various organic changes in the brain, a great diversity of lesions, may give rise to nearly identical symptoms, and in the actual state of the science we are compelled to use the utmost reserve whenever we would attempt to explain, by the nature of the lesions found in the dead body, the functional derangements presented during life by the brain or its appendages.

You need not go deeply into the history of cerebral disease to be convinced of this truth. Let us take, for example, two individuals who are labouring under congestion

of the brain and cerebral anemia. The anatomical characters of these two affections are certainly quite distinct, but are they expressed in the living body by a corresponding difference of symptoms? Unfortunately, experience says "No." In many cases where the patient has been affected with delirium, with convulsions, with loss of sense, and, finally, with complete coma,—an assemblage of symptoms that ordinarily points out an increased determination of blood to the brain,—we have found after death a complete decoloration of the cerebral mass, particularly the gray substance, and a correct diagnosis can in these cases only be established, not by the present state of the patient, but by the antecedent history of his disease. We have there a variety of lesions in the brain and nervous system, manifesting themselves externally by symptoms so nearly identical, that in many cases it is almost impossible to connect the symptoms with any determinate organic lesion. But this is not all; we have, on the other hand, changes of structure identical in their nature, which give rise to various and almost contradictory symptoms, producing by this want of harmony between cause and effect, one of the most marked difficulties which we have to encounter in our investigation of diseases of the nervous system. We are, however, sometimes enabled to explain this apparent want of connection between the trouble in the functional phenomena and the change of organic structure. In some cases this depends on the more or less rapid formation of the disease, producing either a number of obscure and unconnected symptoms, or a sudden and profound change in the economy, corresponding with the rapidity with which the organic lesion itself has taken place. In other cases the variety of symptoms presenting themselves with the same lesion, may depend on the difference of the seat of the lesion in the nervous system. Let us suppose, for example, that the disease commences as ramollissement; that we have two patients, one affected with softening of the cerebral hemispheres, the other with ramollissement of some part of the cerebellum. In the two cases just mentioned, we shall have a difference of symptoms, although the nature and extent of the original lesion may be exactly identical, and this depends on the circumstance, now well established, that the brain is not a simple organ, but is composed of several different parts, each of which presides over a different function, and regulates the vital properties of distinct portions of the frame. The phenomena which we observe during health, and, perhaps, still more, the functional derangement occurring during the progress of cerebral and nervous diseases, prevent us, I say, from adopting the idea that the brain can be a single organ; but, on the other hand, numerous as the observations have been, which tend to establish the locality of several functions, I do not think that we have as yet arrived at a point where we can say with any degree of certainty, that such or such portion of the brain presides over such or such a function. We are still, gentlemen, unable to point out the precise spot in the brain to which movement is subservient, which directs the complicated phenomena of memory, or gives birth to the still more wonderful and noble manifestations of the human intellect. The time of this discovery, which will doubtless throw an immense light on the history of diseases of the brain, is not yet come; let us hope that it may not be far off, and that any premature attempts to localize the cerebral functions may not be injurious to its progress. In admitting, however, the existence of different parts in the brain, destined to the production of so many distinct functions, we are at the same time compelled to allow the existence of some principle which gives an *ensemble* to these various parts, which harmonizes and unites them into one whole, which, in a word, produces a cerebral, as well as a vital unity.

The several causes of difficulty, gentlemen, to which I have just now directed your attention, may be classed under certain heads, and, in some measure, reduced to fixed laws; but we have other obstacles of a different nature, that are, perhaps, not less embarrassing. You are all, doubtless, acquainted with the varieties in the symptoms of disease, produced by what is called "idiosyncracy," or individual temperament. Thus we may have two stomachs affected with gastritis, two lungs affected to the same extent with tubercular disease, and yet the signs will vary considerably in both cases. One patient may present all the symptoms which characterize the malady in a high degree, while many of the principal signs may be absent in the other, and *vice versa*.

The same modifying influence of temperament is seen, perhaps even to a greater extent, in diseases of the brain and nerves, where the same lesion, identical in nature, in seat, and in magnitude, often produces the most varied symptoms. Thus in one patient it will be accompanied with little or no trouble of the cerebral functions: in another, placed apparently under similar circumstances, the same change of organization is accompanied with accidents of the most alarming nature. Thus also we have seen very severe lesions, such as enormous abscess of the substance of the brain, tumours, &c., occurring without any perceptible lesion of the intellectual faculties, while in other cases the same pathological condition is attended with delirium, or even complete destruction of the mental power. Cases of this kind (and they are far from being extremely rare) cannot be explained by any

of the known laws of organized matter, and we are compelled to have recourse to peculiar idiosyncrasies, a supposition by which we are not much advanced in the investigation of disease.

I have now laid before you a certain number of the difficulties which we have to encounter in the study of cerebral and nervous maladies; but these are far from being all that you will meet with. In speaking of the causes which determine diseases in general, I mentioned to you how easily the different organs of the human body acted mutually upon one another; how quickly disordered function, or organic lesion of one part or apparatus, was felt by some remote organ, or by the economy at large. In the nervous system this susceptibility becomes a source of great and peculiar difficulty; for we observe no disease of any importance, no functional derangement of any length in the great organs, or even in those less intimately connected with the support of life, without at the same time observing a variety of derangements in the nervous system; and it is frequently a matter of the utmost difficulty to decide whether the trouble in the functions of the system be primary, essentially connected with some change of organization, or merely a secondary affection, produced by the local disease, kept up under its influence, and ready to disappear with it. This is a question which every physician is daily compelled to ask himself at the bed-side of the patient, and which even the most enlightened and experienced practitioner is not always in a condition to resolve.

I observed to you, gentlemen, in an early part of this lecture, that diseases of the nervous system exhibit symptoms which vary considerably, according to the age of the individual attacked. This truth has long been confirmed by experience; you may establish its exactitude for yourselves. Go to the hospital *Des Enfans Malades*, frequent the wards of *La Pitie*, or visit occasionally *Salpetriere;* examine for yourselves the cases which present themselves in those establishments, follow the autopsies, and you will soon be convinced that diseases which are identical in their anatomical characters may exhibit symptoms of a very varying nature, and undergo several most important modifications according to the age of the individual.

The greater part of the difficulties in the study of diseases of the nervous system which we have now noticed, do not exist with respect to affections of the other systems, or are met with in a much less degree; we have thought it right to lay them before you in some detail, that you may be the more fully aware of the obstacles which you have to encounter during the pursuit of your studies in this branch of medicine.

When we have once determined the seat of a disease, and the symptoms by which its existence is accompanied, our next care should be to examine the lesions which the disease may leave behind it, to fix the nature of those organic changes that have given rise to the functional derangement observed during life. During the last few years, certainly, the anatomical pathology of the brain has made an immense progress; various morbid alterations before unknown, have been studied, and their nature investigated with the most happy results. I need only refer you, in proof of this, to the minute and laborious researches of Messrs. Lallemand, Rostan, Bouillaud, &c., on ramollissement of the brain; but, notwithstanding the works of these and other distinguished pathologists, much more yet remains to be achieved, and the investigation of the morbid changes which take place in the nervous system, affords a wide field for discovery. We must not conceal from ourselves the fact that our researches in this part of morbid anatomy are surrounded with obstacles and difficulties. In the other organs of the human body the principal lesions are attended with appreciable changes of structure; for each deviation of function, we can find an alteration in the physical properties of the tissues, which in a great measure explains the disordered manifestation. If then the variety of functional disorder in an organ has a constant relation to the variety of disorder in its texture, no part of the body should present a greater number of lesions than the nervous system; but this is far, very far, from being the case. Indeed we may lay down this medical paradox, that the number of diseases of the nervous system which are unattended with any change of structure whatever, infinitely surpasses the small number in which we can find some appreciable sign after death. This assertion may at first sight appear startling, but it is perfectly confirmed by experience. In fact, the number of lesions peculiar to the nervous system is very small, and what is more embarrassing, they often bear no connection whatever with the nature or intensity of the symptoms. Consider, for a moment, the chief and most frequent of these lesions;—apoplexy, softening of the brain, meningitis, and a few others; but these do not constitute all the diseases of the nervous system, although they may comprise the greater part of its pathology. No, gentlemen, we have an immense number of other nervous diseases, and if you ask the anatomist where they are, he is compelled to confess that here the science is at fault; in a word, that in a great majority of cases we are unable to find any change in the nervous centre or its branches, although their functions may have been deranged in the most marked and dangerous manner during life. I therefore repeat, that in a vast majority of cases the

examination of the dead body cannot explain the various phenomena we observe during diseases of the nervous system; and if we place too exclusive a reliance upon pathological researches, we shall become involved in contradictions, from which escape is very difficult.

If we look for the cause of this slow advancement of the science in diseases of the brain, we may perhaps find it in the circumstance that a great number of functional disorders may really exist without any change of structure sufficiently pronounced to be observed by our senses after death. This, I say, may possibly be the case; the lesion of innervation may be sufficient alone to produce various troubles in the economy, without being accompanied by any organic derangement of tissue; however, we should be inclined to suppose the contrary from analogy. These lesions very probably do exist, although they have hitherto escaped all our researches. We have, therefore, the greater need to increase our diligence, multiply our observations, and examine with most minute attention the slightest change not only in the arrangement and texture of the nervous system, but also in its composition, and at the end we shall perhaps arrive at truths whose discovery is now almost unlooked-for.

We are indebted, as I said before, highly indebted, to pathological anatomy for some of the most important discoveries in medicine which have been made in modern times; but it is unfortunately an effect of the ardour with which one branch of the science is pursued, that many other considerations, of minor importance indeed, but still necessary to the full knowledge of disease, have been comparatively neglected. Thus, perhaps, we have not paid sufficient attention to the relative development of the several parts which, taken together, constitute the *ensemble* of the nervous system. Almost exclusively devoted to the research of changes in the texture of organs, we have neglected to inquire in what manner an irregular development of certain portions of the brain may be a cause of trouble to the functional actions of the whole organ. We have many reasons for believing that loss of equilibrium in the normal growth of the different portions of the cerebral mass, may become the cause of deranged equilibrium of its functions, just as well as if the change were operated in the structure of the part. I merely throw out this idea as a matter for your reflection; the time for discussing the question has not yet come. Again, there are certain changes of nutrition, certain phenomena connected with the chemical composition of the brain, which may not be without influence on its functional manifestations, and therefore deserve our attention while examining the history of disease in an organ where it is so difficult to trace after death the physical changes that may have been the cause or accompaniment of the original malady. In latter times, gentlemen, it has been proved that the elements which compose the brain vary from one period of life to another. Thus in the infant and child the quantity of aqueous matter predominates over the albuminous, and the brain itself presents an augmentation of volume from birth up to the age of puberty. In the adult, again, the quantity of phosphorus which enters as a component into the chemical composition of the brain, is greater than in the child or in the old person. The quantity of phosphoric matter, inconsiderable in the child, acquires its maximum in the adult, and again diminishes in old age. During the latter period of life the brain shows evident signs of diminished nutrition; its several diameters are reduced by some lines, and its specific gravity is from one-twentieth to one-fifteenth less than in the adult.

These are questions which as yet have only been touched upon as connected with diseases of the brain, but they deserve our serious attention. In the absence of anatomical lesions, it is not altogether irrational to conclude, that perhaps some diseases of the nervous system may be explained by chemical changes in the substance and complication of the brain; but this only proves that at a future time we may be in a condition to give a satisfactory reason for several phenomena which are now involved in total obscurity. The ideas which I have just thrown out upon this point, are mere suppositions, that may be confirmed or overthrown by subsequent observations; it is as such that I would wish you to receive them, and not as facts that obtain the value of demonstrations.

Let us now turn to another part of the subject, and consider, briefly, diseases of the nervous system in a therapeutic point of view. Here also you will find yourselves beset by many unexpected difficulties. In various abnormal conditions of the nervous system, the patient shows marks of excitement, the energy of the nervous force appears elevated, and the symptoms seem to indicate such means as usually operate a diminution of the vital power; but in these cases, if you employ blood-letting to any extent, instead of allaying the nervous trouble you aggravate it, and the indications of treatment upon which you had reliance in disorders of the other systems, here leave you in fault. This is particularly seen in many of that class of diseases called "neuroses," where sanguineous emissions always exasperate the symptoms of exaltation which seemed to indicate them, and where we are compelled to have recourse to quite an opposite system of treatment. Again, in many intense inflammations of the nervous centres, where the only hope of safety apparently

resides in copious and repeated bleeding, this latter measure is attended with dededly injurious effects; the excitement, instead of being calmed by the loss of blood, rapidly increases, and is soon followed by a fatal exhaustion of the vital power.

In many other diseases, also, some peculiar nervous state exists, which will not admit of blood-letting. In chlorosis, for example, we have signs of a nervous exaltation; but these are dissipated under the use of quinine, of cold effusions, of strengthening diet, exercise, &c., which bring back the harmony between the functions of the several parts of the nervous system, while sanguineous emissions would, on the contrary, only tend to keep up the disorder. Another difficulty in the appreciation of the effects of remedial agents in disorders of the nervous system, is the immense and varied power enjoyed by the imagination over almost all our functions; the force of a vivid imagination, the power of energetic belief, the influence of religious or superstitious impressions, are capable of producing and curing diseases, not only such as depend upon derangements of the nervous system, but others which have taken much more profound root in the economy. Of this, history affords us proof every day, without ascending to a period of remote antiquity, or the miracles of the royal touch. How easily can we explain by the imagination, many of the cures obtained from the magnetisms of MESMER, down to the infinitisms of homœopathy! It is the influence of this same imagination which constitutes the force of the charlatan, working a cure by invisible means, which is attributed to the nullities he administers: in a word, the influence of the imagination on the normal and abnormal conditions of the human body, forms one of the most curious parts of the history of man.

There are, perhaps, few professions which require a greater combination of varied and extensive knowledge, than that of the physician; and if this be true for the study of disease in general, we feel a still greater necessity, when entering on the investigation of nervous diseases, of preparing and clearing our way by a previous study of the other accessory sciences. Above all things, gentlemen, your efforts should be directed to the acquisition of anatomical knowledge; not of the rough imperfect anatomy with which you are contented in the commencement of your education, but of minute and delicate anatomy, which alone can enable us to throw any light on the changes of organization that take place in the brain and nerves. You will find this minute research of the most precious aid in your investigations, indeed it is the only method by which we can hope to arrive at an interpretation of the numerous facts we already possess, without being able to give any explanation of them in the actual state of the science. Cultivate then, I say again, minute anatomy, and believe me that if you would pretend to advance this obscure portion of medicine, you must, above all things, be a profound anatomist.

The knowledge to be derived from comparative anatomy and from experimental physiology is also not to be neglected. In some cases you will observe the movements of the lower extremities extensively lost, though the lesion in the spinal marrow is comparatively insignificant; in other cases the loss of motion and sensation is less perfect, although the injury of the spinal marrow may be carried to such an extent as to cause its complete separation into two portions: it is difficult to understand how this can take place, but without attempting altogether to explain it, I may observe, that in some fishes I have seen the nerves completely separated from the spinal marrow, and yet sensation and motion have been conveyed from it to the distant parts of the body, through a fluid.

We can also derive considerable assistance towards the clearing up of certain obscure points in pathology, from physiological experiments; thus we have been able to understand and explain a great number of interesting points connected with paralysis of the face, only since the period that experiments have demonstrated the different functions attached to the fifth and seventh nerves. Again, consider the great light which experimental physiology has thrown on the pathology of the spinal marrow; in man we have frequent examples of an injury affecting only the anterior columns of the spinal marrow, and the functional derangement is betrayed by a greater or less loss of the motile power; in other cases the lesion is situated chiefly in the posterior columns, and here sensation is the function which suffers. How could we explain this difference without the beautiful discoveries of some modern physiologists, who have shown that the posterior columns of the spinal marrow preside over sensation, while its anterior portion is chiefly destined to regulate sensation!

The difficulty of tracing the morbid phenomena presenting themselves during life, to certain organic lesions, may, in some cases, perhaps, depend upon our not searching for the lesion in the place where it really exists, and this often, in turn, depends upon a neglect of physiology. Thus, in many instances, a patient goes blind without any sensible alteration in the structure of the eye. After death we examine the optic nerves, their commissure, the thalami, in a word every portion of the brain supposed to bear any relation to the sense of vision; and we find all these parts equally free from any appreciable lesion. In another case the sense of hearing is deranged, or altogether lost. We examine the internal ear, the tra-

ject of the portio mollis, the striæ in the floor of the fourth ventricle, and every thing presents a normal appearance: are we therefore justified in concluding that the loss of sight and hearing in these cases did not depend upon change of structure in the nervous system? I do not think so, gentlemen; we have omitted in our investigations an essential element; experiments have demonstrated that the fifth pair of nerves is an accessory nerve to the functions of the different senses; nay, more, comparative anatomy teaches us that the branches of the fifth pair may themselves become nerves of sensation in certain animals who possess organs of sense that do not exist in man, or in other animals placed low in the scale of creation, this accessory nerve actually becomes a sensitive nerve, and supplies the organs of sight and hearing: hence it is by no means improbable that in many cases of blindness and deafness, the loss of those two functions may depend upon a lesion of the fifth pair of nerves, or some of its branches, and not upon an injury to the principal nerve distributed to the diseased organ.

Thus, gentlemen, you see how closely these different branches of knowledge are connected together; how pathology is aided by comparative anatomy and experimental physiology, and how necessary it is to cultivate these latter branches of the science if we would hope to take a distinguished part in the advancement of the former.

I have thought these considerations upon the various difficulties that present themselves in the study of nervous diseases, of sufficient importance to lay them before you, gentlemen, in some detail. Let us now explain, briefly, the order we propose to follow in the present course. We shall first describe the different lesions of the central nervous system, commencing with the most simple forms, and thence passing to the more complicated; we shall then take up lesions of the nerves; and, finally, terminate with a review of those disorders which are generally attributed to a lesion of the great sympathetic nerve. We have already seen, as regards the other principal systems of the economy, that diseases may be distinguished into *Lesions of Function*, and *Lesions of Structure*. The same principle of division holds good with respect to the brain, and we shall soon have occasion to demonstrate how the influence of this organ extends to all parts of the economy, modifying disease in a peculiar manner, and stamping all with a characteristic mark.

Finally, with regard to the spirit in which we propose to conduct the present course, we may say that its chief object shall be a just appreciation of facts. Some of these facts are premature, and as such cannot be taken into account; others are demonstrable, and upon those principally shall our deductions be founded; others, again, are merely probable, but are by no means to be neglected from that circumstance. We are not of opinion with those who say that a probable fact is no fact at all, "*un fait vraisemblable est un fait non avenu.*" On the contrary, many of these possess a very great value, and instead of turning from them with neglect, we should endeavour to verify and appreciate them. Lastly, some facts are false and inaccurate; but even here I would remark to you that many facts are supposed to be so, merely because they do not agree with our own theories, with the ideas we have been taught to adopt; you must, therefore, weigh each fact with care and impartiality before you decide on its rejection, for nothing is more fatal to the science than that absurd cynicism which believes nothing that it does not see. Our last care, gentlemen, shall be to notice some of the principal theories which prevail with regard to disorders of the nervous system. We cannot pretend to expose all; we shall, therefore, confine our attention to the most plausible. I am not one of those who run after hypotheses and theories, but as a teacher it is my duty to lay them before you, and thus keep pace with the progress of the science, for we have often seen the most interesting and important discoveries, whose authors commenced by inventing hypotheses, and terminated by proving them.

ON THE

NATURE AND QUALITIES OF FLAME.

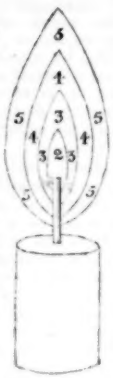
By W. Reid Clanny, M.D.,

Sunderland.

When a simple body is burnt, the flame is uniform throughout; but when compound bodies are burnt, the flame varies considerably, depending upon the nature of the combined gaseous substances inflamed, and producing a variety in the intensity and colour of the light.

As a familiar example, I will commence by giving a correct account of the phenomena of the flame afforded by a candle, and by way of explanation, append a diagram of that flame, afterwards describing the experiments which led me to the conclusions I have adopted, and the apparatus which I employed.

Surrounding the wick, and below the



Arabic characters (3.2.3), we always find a clear blue flame which chars the wick from the moment it is lighted, and in forming a base for the light-giving flame, at this point, the two flames show a blue-white light at their juncture.

Immediately surrounding the top of the wick we find a conical space (2), which forms a centre to the yellow-white flame, and which is less transparent and less luminous than the yellow-white flame, immediately surrounding it.

Round the space (2) we observe the yellow-white or light-giving portion of the flame, which is also conical (3.3.3), as all heated gases are in ascending, by reason of the uniform pressure of the atmosphere.

Surrounding the last-mentioned light-giving atmosphere (4.4.4) we find an atmosphere which is, generally speaking, invisible to the eye, except we use a large coarse candle in a very dark room. This atmosphere is diaphanous, and surrounds the light-giving flame at every point.

Externally to this invisible flame we find another still (5.5.5), which is composed of nitrogen and carbonic acid gas, but as, of this atmosphere, and of all other parts of the flame, it will be my duty to treat more at large, I will commence by stating, that when we hold a piece of fine brass wire across the flame of any description of candle or oil-lamp, the atmosphere (5.5.5) does not effect any change in its appearance. In the atmosphere (4.4.4) which, as remarked above, is also invisible, we find that the wire instantly becomes yellow-white, from the great heat afforded by this invisible flame.

Immediately within this invisible flame we find (3.3.3) the light-giving flame, and at this film, or atmosphere, of yellow-white flame, the wire becomes *red*-hot only. In the centre or the obscure part of the flame of the candle (2), we do not find that the brass wire is so much heated as to give it the tint of what is called "*cherry*-red." Indeed, I am of opinion that were it not for the conducting power of the wire itself, this obscure portion would not give to it any appreciable augmentation of heat; for, assuredly, this opaque or central portion of the flame, is *not* in a state of combustion, as I expect to be able, very shortly, to demonstrate. When we place before us a candle of any description which has been burnt for some minutes, and extinguished in the usual manner, we find, on holding any flame to the extinguished wick, that the first appearance of ignition is a clear blue flame, which I have discovered by experiments to be carbonic oxide gas. This blue flame, as it continues to burn, will augment in extent, and soon afterwards will show a white flame upon the top. This white flame will gradually augment until it become the light-giving flame of the candle.

The base of blue flame gives out a powerful heat, which not only chars the wick, but also melts the tallow of the candle, and this melted tallow is pumped up, as it were, in the heated interstices of the wick in the manner of fluids in all capillary tubes. At the top of the ignited wick we may readily observe, without the aid of a lens, the inflammable gases springing up into the centre or opaque part of the flame (2), which may be seen more readily after the wick has been snuffed. This liquid or melted tallow, being composed of carbon, hydrogen, and oxygen gases, is *manufactured* by the heat of the flame, and becomes an empyreumatic oil,—olefiant gas, bi-hydruret of carbon, and carbonic oxide gases, suspending a considerable portion of free carbon; and these substances, when in a state of combustion, we call "flame."

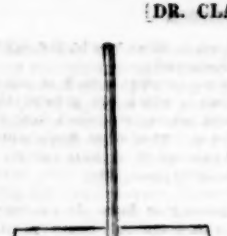
Carbonic oxide gas is formed whenever carbon is burnt without its receiving the needful quantity of oxygen gas to cause it to pass into carbonic acid gas. Carbonic oxide gas is remarkably inflammable, and takes fire in the open air when it comes into contact with iron at a cherry-red heat. It is, therefore, in the first instance, burnt at the base of all flames into the composition of the gases of which it enters, as we may observe in the familiar instances of lighted candles, oil-lamps, and gas-lights.

When the free carbon is in excess, in the *pabulum flammæ*, a portion of it is, during combustion, attracted into the body of the blue or carbonic oxide flame, and there contributes, in a very beautiful manner, to the augmentation of the light at the base of the flame.

When carbonic oxide gas is burnt by itself in the open air, it has, also, its invisible flame, and over this there is an atmosphere containing carbonic acid gas. The invisible flame of carbonic oxide gas has exactly the same properties as the white or light-giving flame when it is burning in a cylinder of wire gauze, except that no water is formed in the combustion, as hydrogen gas is not present.

I now enter upon the subject of inquiry in respect to the nature of the opaque portion of the flame, marked 2 in the diagram.

Into a piece of wire gauze (*vide* diagram) I inserted a brass tube four inches long, and the eighth of an inch in caliber.



It will readily be understood that when I held the piece of wire gauze horizontally, so as to cut off and consequently extinguish the upper half of the flame, the centre or opaque part of inflammable gases not being ignited, would be conducted up the brass tube in the manner of a chimney. When this was neatly and steadily performed for a minute, I could readily ignite these gases as they sprung from the centre of the flame, and I always observed that the flame at the top of the tube was remarkably brilliant. Upon conducting these gases into a tube of glass open at top and bottom, I was enabled to collect a sufficient quantity thereof for experiment, and I found that it consisted of the *pabulum flammæ*, above mentioned,—that generally the olefiant gas was in excess, and hence the strength and beauty of the flame. In all instances I found a considerable quantity of free carbon suspended in these gases. I found no oxygen gas in this opaque portion of the flame. Indeed, it could not be expected to be found there, because, as I have stated above, this portion of the flame is not in a state of ignition (which would not be the case did it contain oxygen), and because in the combustion in the body of the wick, which converts it into a state of charcoal, all the oxygen contained in the tallow, wax, or oil, is expended in this combustion.

We now come to the yellow-white or light-giving portion of the flame (3. 3. 3). This flame surrounds the opaque or central portion of the flame, except at that part at which it comes into contact with the base of blue flame. This flame, as may now be readily understood, is composed of the substances above-mentioned contained in the centre or opaque portion of the flame. In a word, this is the only true light-giving portion of all flames, and has its dependence upon the invisible atmosphere (4. 4. 4) which surrounds it, and to which I must now advert.

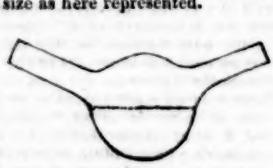
Surrounding the light-giving flame, and the base of blue flame, we always find an invisible flame. This invisible flame will, in some degree, become visible when we cut a black card into the exact form and size of the light-giving flame, and, in an otherwise dark room, completely eclipse the light-giving flame. In such case, though the flame be scarcely visible, we may observe that it is uniform throughout, and is about the eighth of an inch in thickness. This invisible flame is of the greatest importance, for without it we should have *no flame whatever*.

From numerous experiments with the wire-gauze and brass-tube apparatus, I am satisfied that, at every point, highly heated free carbon is attracted from light-giving flame, and is burnt in the atmospherical air surrounding the flame on all sides, and which air is at all times moving in a current over the flame, from below upward, so that as fast as the atmospherical air affords sufficient oxygen for feeding this invisible flame, it, viz. the heated surrounding atmosphere, is carried upwards, by reason of its expanded state, effected by the intense heat at all times given out by this invisible flame.

This invisible flame instantly fuses fine brass wire, which the white or light-giving portion, in some instances, requires a considerable time to accomplish. I am well assured that this invisible flame is perfect throughout, as I have examined it carefully with powerful lenses; and, also, that by the great heat which it gives out, the inflammable gases of every description are burnt, and that without it, flames of every description could not continue for three seconds.

When we blow upon any flame, with *heated* air, we drive away this invisible or heat-giving flame, and if we continue to blow, we next drive off the white flame, and, lastly, the centre, or magazine of inflammable gases, and thus mechanically extinguish the flame, but not by the current of "*cold* air," as Davy and others after him supposed. Of this more hereafter.

Over the invisible flame we have another atmosphere (5. 5. 5. 5), of which I will now give some account. The smallest engraving represents a glass apparatus exactly of the same size as here represented.



This glass apparatus is composed of two tubes, which project from a bulb, into the latter of which lime-water is poured (by a small funnel), which is represented by the dividing line. I hold the aperture of one of the tubes to any part outside the invisible flame, in such a manner that a current shall pass through the little apparatus from the flame, outwards, taking care that the lime-

water remain steady in the glass bulb. I allow this current to be established for a few seconds, and then place the fore-finger of the right hand upon the aperture of the outward tube, and the thumb of the same hand upon the aperture of the tube which had been next to the flame. I now very cautiously agitate the lime-water contained in the bulb. In all instances I have uniformly found, that the lime held in solution in the lime-water became visible as a carbonate, by reason of its union with the carbonic-acid gas, conveyed by this glass apparatus from the atmosphere immediately surrounding the invisible flame. This atmosphere of carbonic-acid gas I estimate at one-tenth of an inch in thickness.

As mentioned above, this atmosphere is carried upwards as fast as it is formed, for the reasons there given. Hence the necessity of the constant supply of oxygen gas which our atmosphere affords, and hence the brilliant combustion which takes place when inflammable substances are burnt in oxygen gas.

The invisible flame is destroyed when we hold any incombustible substance against it, and, consequently, the yellow flame recedes at that point, or, rather, is destroyed. Instantly afterwards a new invisible flame is formed, and when we press the incombustible substance forward at that place, the yellow or light-giving flame is again destroyed, and so on until the light be totally extinguished or destroyed. When we arrive at the opaque or centre portion of the flame, we shall find that a considerable portion of free carbon will be precipitated upon the incombustible agent. When we place an extinguisher upon any flame, we will find that it does not come into contact with the flame, even though it be extinguished, because a sufficient portion of oxygen gas is not allowed for the formation of the invisible flame. These phenomena take place with flames of all description into which carbon enters. I need scarcely remark that the purer the atmospheric air, and the greater the portion of carbon which enters into the composition of the above-named gases, the more powerful, *cæteris paribus*, will be the *combustion* or *explosion* of those gases.

When we hold a piece of wire-gauze web, of from 28 to 30, both warp and woof (such as Sir H. Davy recommended to be employed in his safety-lamp), to any flame in which carbon is burnt, we shall thereby prevent the formation of the invisible flame within the wire gauze, and another invisible flame will form nearer the centre of the light-giving flame. If we now follow the light-giving flame with the wire-gauze web, we shall find that it will again recede from the wire gauze, and form a new invisible flame; and should we persevere, we shall find that the light-giving flame will ultimately be extinguished, or, in other words, the light will be totally destroyed.

When we employ a web of wire gauze, the meshes of which are greater than that mentioned above, we shall find that the flame (no matter of what description it may be) will *pass* the wire-gauze barrier.

Sir H. Davy, and, after him, many eminent chemists, supposed that the prevention of the passing of flame by the wire gauze which he employed in his safety-lamps, was effected by the "cooling power" of the wire gauze, but this is not the case, as my experiments in this paper testify.

We know, from what I have stated above, that if the invisible flame be prevented from being formed by the above-named agents, so that there may be a deficiency of oxygen gas in Davy's safety-lamp, combustion cannot be continued, and hence the *comparative* safety afforded by this lamp. But it is needful to assert and maintain that the formation or nonformation of this invisible flame accounts for all the phenomena which that distinguished philosopher referred to the cooling power of the wire gauze.

The more this position is known, and, indeed, I may say, the more that my experiments are tried by competent judges, the greater assurance of their correctness will, I trust, be entertained.

Having visited several very "fiery coal-mines," I am assured that when pitmen are hewing coals they seldom cast an eye to their safety-lamps, and if they did the dense atmosphere of coal-dust would prevent their observing the phenomenon of fire-damp burning in the safety-lamps, as, in fact, there is, in nineteen cases out of twenty, no augmentation of light when fire-damp burns in a Davy-lamp, as I have had reason frequently to remark. Besides, if fire-damp burn several times in a Davy-lamp, the wire gauze becomes oxydated, and, falling in pieces, cannot prevent the flame of fire-damp from passing. Should an accident, such as any pitman's working-tool, unwittingly penetrate the cage of wire gauze in the Davy-lamp, during the prevalence of fire-damp in a coal-mine, the flame of course would pass thereby into the atmosphere of the mine; or should any part of the wire cage be rusted, the fire-damp would soon pass at that part, and, in such cases, explosion of the coal-mine would follow. Another source of danger arises from the following cause:—When the fire-damp is mixed with very pure atmospherical air in the coal-mine, the flame in the Davy-lamp burns so steadily, and with such power, that should the pitman either inadvertently, or through fear, run quickly along the gallery of the mine, we know that the flame of fire-damp would in such case pass out from the wire-gauze cage of the Davy-lamp. It will not appear presumptuous in me to assert that my newly-invented apparatus, being exceed-

ingly simple and easily to be understood, will obviate the dangers pointed out above, as I have certificates from several of the most respectable persons concerned in coal-mining, who spontaneously conferred these kindnesses upon me. Nor have I in any instance experienced the least opposition to my views, and the application of my discoveries, for preventing accidents. Several eminent persons concerned in the management of coal-mines, and who have not yet tried my new safety-lamps, have given me upon inspection their approval of them in the most satisfactory terms, and I have reason to expect that in addition to those first-rate coal-mines in which my safety-lamps have been tried, approved, and used effectually, managers of other coal-mines will, as may seem suitable to their convenience, adopt them. I may remark, *en passant*, that when the wire gauze in a Davy-lamp is more open in its texture than in the proportion of twenty-eight to thirty, in both warp and woof, as Davy himself proved, we shall find that the invisible flame will, by such apertures or meshes, be permitted to be formed outside the wire-gauze cylinder, which cylinder, in that case, cannot be considered a barrier to the progress of flame. Here, "cooling power" is out of the question, and, as in all other cases, does not afford the true explanation of the phenomenon.

As I have not drawn up this paper for the perusal of the learned readers of THE LANCET, in order to show the value of my new safety-lamp, but for the advancement of science, I will conclude by remarking, that by the use of my new safety apparatus, all the contingencies which are mentioned above in respect to the Davy may be avoided; and as the Davy is in general use in the coal-mines of the north of England, I considered it imperative upon me to add to the Davy in use the safety shield, so that the expense of new safety-lamps may be avoided. I am informed by all persons using my safety-lamps, that the wire which I employ for supporting the safety cylinders, *stands* to their satisfaction, when placed over the flame of the oil-lamp in the wire cage, but the moment fire-damp burns within the cylinder of wire gauze, the piece of fine wire is fused, and the whole cage is instantly surrounded by the safety cylinders.

Sunderland, Nov. 5. 1835.

THE printed Minutes of Evidence, and the Report of the Parliamentary Committee appointed to inquire into the lamentable catastrophes which are so constantly occurring in the *Mines* of Great Britain, with the view of ascertaining the means of preventing the recurrence of similar accidents, have just been issued. We shall shortly examine some parts of the document, which extends to 360 folio pages.

ON THE

LAW OF MORTALITY

IN

EACH COUNTY OF ENGLAND.

By T. R. EDMONDS, Esq., *B.A., of Trinity College, Cambridge.*

THE knowledge of the laws which regulate the collective vitality at different ages of the population in various districts of England, is evidently an object of the highest interest to physiologists. When those laws are established, and when the peculiar circumstances of each locality have been investigated, we may reasonably expect soon to arrive at a knowledge of some of the chief causes affecting the prolongation of human life. Having minutely examined the great mass of facts accumulated in the *English Population Returns* of the years 1821 and 1831, I can speak with confidence of the high value of the information which they contain. The results from *one hundred* separate observations of males and females, are consistent with each other, and in harmony with previous observations, when viewed in connection with the new theory of mortality which I have explained in Nos. 605 and 614 of THE LANCET. There exist, however, in different localities, considerable variations in the mortality at different ages, and in the relative mortality of the two sexes. The causes of these variations, whether dependent on atmospheric or other external circumstances, or whether dependent on original peculiarities of individual constitution, is a subject well worthy to exercise the ingenuity of physiologists.

Before proceeding to the general investigation of the subject, it may be interesting to state a few of the principal results hereafter to be established. In comparing together different counties, the characteristics of each peculiar law of mortality are marked by the mortality in three principal divisions of age; namely, in the period under five years of age, in the period between fifteen and sixty, and in the period above sixty years of age. In all counties, out of a given number living above the age of sixty years, the number dying in one year is nearly the same. In some counties the mortality between the ages of fifteen and sixty years is nearly half as much greater than in many other counties. In some counties, out of a given number born, nearly twice as many die under the age of five years as in some other counties. In all counties the mortality of males under the age of five years exceeds the mortality of females at the same age, in the proportion of seven to six nearly.

In the majority of counties, at ages greater than sixty years, the mortality of males seldom exceeds that of females more than three per cent. In the majority of counties the mortality of males between the ages of fifteen and sixty years is *less*, in a small degree, than the mortality of females. The most remarkable result presented by the population returns, is the fact that all the counties wherein the mortality of females between the ages of fifteen and sixty years is at a maximum, are found on or near the same straight line. A low mortality between the ages of fifteen and sixty years is the best index of a healthy population, because the individuals of this class are of much higher political value than the individuals under five years or above sixty years of age. As an index to the healthfulness of a locality, the law of female mortality is to be preferred to that of males, because the various occupations of the latter may exercise an important influence on the mortality. The line of greatest mortality of females between the ages of fifteen and sixty years, is a central one, running in a north-west direction from Brighton to Liverpool. The most healthy counties are those most distant from this line, with very few exceptions.

The materials on which the present observation is founded consist in a return of the ages of the *dying* in all England and Wales during the eighteen years 1813-30, combined with one enumeration of the *living*, and their ages made in the middle of the year 1821. The only important defect in these materials is the omission of a considerable and uncertain number of deaths. The returns made show the number of deaths which have been entered in the parish registers only; and we are left with very little information respecting the numbers omitted through negligence, or omitted because buried in the grounds of dissenters, or in private burial-grounds. Mr. Rickman, who compiled the returns, estimates the deficiency in the deaths to be 8 per cent. less than the true number for the whole of England and Wales. The results which I am about to give for thirty-nine counties of England, are founded upon the assumption that the registered deaths are deficient ten per cent. from the truth. I have supposed that, out of every twenty deaths which have occurred, one is omitted through negligence, and another because the burial occurred out of the church grounds. In the counties of Middlesex, Surrey, Monmouth, and in Wales, I have estimated the deficiency at 20 per cent.; Mr. Rickman having expressed his opinion that the deaths are more deficient in these than in other districts. The consequent deficiency for the whole of England and Wales will then amount to 12¼ per cent. nearly, instead of 8 per cent., the estimate of Mr. Rickman. The number of deaths which I have assumed as the true total, differs from the total assumed by Mr. Rickman only one-twentieth part.

It is certainly highly desirable that the number representing the absolute annual mortality at all ages should be correctly ascertained. I believe it to have been ascertained within 5 per cent. of the truth. But even if it should prove defective to the amount of ten per cent., the value of the results hereafter stated will be very little affected thereby: for they would in that case equally well indicate the *relative* mortality at different ages of life, and in different localities, which is the only important question to physiologists. The materials supplied enable us to establish, beyond dispute, the relation subsisting between the mortality at one interval of age, and the mortality at every other interval of age. If the absolute mortality at any one age, or at all ages, in the aggregate be known, the absolute mortality at every interval of age is also known.

In order to obtain the law of mortality prevailing in each county of England during the eighteen years 1813-30, the facts requisite are, the total number of deaths occurring during that period, distributed in quinquennial or decennial gradations of age, together with the mean population who have been alive during this period also distributed according to the same intervals of age. Dividing the number who have *died* in any interval by the mean number who have *lived* in that interval, we obtain the exact number who have died in eighteen years out of a given number constantly living in that interval. A series of numbers thus obtained for each consecutive interval of age would represent the true law of mortality of the population observed. Dividing the results by eighteen, we should have the law expressed for one year, in which form it is most easily compared with the results of other observations. If the results be also multiplied by 100, we should arrive at that form of expressing the fact which is adopted by the best authorities. For example, in the following table the mortality of females in England and Wales, during the eighteen years 1813-30, between the ages of fifty and sixty years, is stated to be 2·16, which is intended to represent the fact that this is the average number of deaths occurring annually for every 100 persons constantly living between the ages of fifty and sixty years. If the materials had been perfect, this number would have resulted from dividing the dying 132,918, by the living 352,160, and dividing the quotient by 18. The result of these operations however is 2·10, which differs from the number above stated, in consequence of necessary correction having been introduced for defects in the materials.

Having stated the nature of the materials as they would be if they were complete, it remains to state the degree and manner in which they are defective from the true

standard. The most important defect, arising from a considerable proportion of deaths not being entered in the parish registers, has been already alluded to. This defect concerns the absolute mortality only at each age, for it may be presumed that these omitted deaths would have been distributed according to age, in the same manner as the total of deaths whose ages are specified. If the defect at all ages be assumed to be 12½ per cent., the defect at each age will also be 12½ per cent. Two minor defects in the materials consist in the want of specification of the ages of a small proportion of the total number returned as living and as dying. The ages of one-eighth of the living population, and of one twenty-fifth part of the registered deaths, have not been specified. The correction necessary on these accounts at each age, is the same as for the total at all ages. In addition to these unavoidable defects, the enumeration of the male sex is defective, by the entire omission of the ages of the military and maritime population, and we have no information given respecting the total numbers of this large class, resident in, or attached to, each county. We are merely informed that the army, navy, &c. belonging to Great Britain, consisted of 300,000 men. This defect ought not to have existed, for it would have been easy to obtain the ages and number of the soldiers and marines resident in England, and attaching them to the summaries of their respective counties. There would probably have existed little difficulty in obtaining a similar enumeration of the sailors belonging to registered vessels. The utility of such an enumeration of seamen would be very little diminished by the fact that the same sailors do not always continue in the same port; because their places are usually supplied by seamen of some other English port, or by foreign seamen. After deducting the sailors and soldiers on foreign stations, I have estimated the amount of that portion which encountered risk of death in England and Wales at 160,000, so distributed that 100,000 are below thirty years of age, and 60,000 between the ages of thirty and sixty years. The effect of this correction, is to reduce the apparent mortality of males at all ages, from 2.23 to 2.17 per cent. per annum.

In the present inquiry, one of the two essential facts to be ascertained, is the mean number who have been alive at each interval of age, during the period of observation, or during the eighteen years 1813-30. The true mean population would certainly be indicated by eighteen annual enumerations of the living at each interval of age. Four enumerations of the living and their ages, made at intervals of six years, would, however, have been amply sufficient, in the opinions of all qualified persons, to determine the true mean population. But in the present case, we have only *one* enumeration of the living, and this made not at the best time, and yet there seems no reason to doubt that it is nearly as valuable as eighteen enumerations would have been. It appears to be a fact founded upon experience, that in any extensive population, the increase or decrease of the number living in any decennial interval of age, is so uniform throughout a period of eighteen years, that the number living in this interval at the end of the ninth year of the observation, is a mean proportional between the number living nine years before, and the living nine years after. The proof of this fact is contained in the English population returns, by means of a supplementary observation of the mortality in England and Wales during a period of seven years, 1818-24. The results of the observation for seven years, agree exactly with the results of the observation for eighteen years, the mean population at the different ages in each case, being assumed to be the number ascertained to be living in the middle of the year 1821, or in the middle of the respective periods of seven and eighteen years. The absolute mortality during the seven years, was 2 per cent. less at every age than the absolute mortality during the period of eighteen years. The relative mortality in the two cases is identical, as it ought to be if the materials of the two observations were complete; and as there is no reason to suspect the mean population for the period of seven years not to have been correctly assumed, there seems to be no room for doubting that the mean population for the longer period of eighteen years, has not also been correctly assumed.

The present observation would have been more complete, if the enumeration of the living, according to age, had been made at the end of the year 1821, which is the middle of the period of observation. The time of enumeration appears to have been fixed on no settled principles, and it seems to have been a mere accident that the time chosen was so near the time at which the enumeration ought to have been made, in order to render the observed number and ages of deaths for eighteen years of the greatest value. The return of births and deaths is made up to the last day of December in each year; and in order to have the means of instituting any accurate comparison between the population living at any time, and the births and deaths, the enumeration ought to have been made at the beginning or end, and not in the middle, of the year. In defence of this acknowledged error, we are told that the middle, of the year was chosen, because the days were longer! If the population had been enumerated at the end of the year 1821, the number living would have been about ¼ per cent. greater than the number given in the return. The correction necessary on this account, reduces the ap-

parent mortality of females at all ages, from 2.083 to 2.067.

The scale of relation connecting together the mortality at different ages in large towns, is of a totally distinct character from that applicable to the general population. The relative mortality of the two sexes is also widely different. In the country, the mortality under the age of five years, is only ten times as great as the mortality between the ages of ten and fifteen years; whilst in large towns, it is fifteen times as great. In the country, above the age of five years, the mortality of males exceeds by a very small amount the mortality of females; and between the ages of ten and fifty, the mortality of females even exceeds that of males. But in large towns at every interval of age, the mortality of males considerably exceeds that of females. For the whole of England and Wales, I have assumed that the registered deaths are to be increased 13.947 per cent. for males, and 13.894 per cent. for females, in order to obtain the true number of deaths. For large towns, I have assumed the necessary increase to be 20 per cent. for each sex; it having been always supposed that the deficiency is considerably greater in towns than in the country. If a lower degree of disparity had been adopted, the minimum mortality, or the mortality between ten and fifteen years of age, would have been less in large towns than in the whole territory. The population returns contain observations on six towns only, which are of the largest size, and which contain a large proportion of sailors and soldiers, of which no enumeration has been made. Having no materials for making a satisfactory estimate of the deficiency, I have made no correction of the apparent results; so that the mortality of the male sex in the six towns between the ages of fifteen and sixty years, is represented considerably higher than the fact. Instead of 3.39 for the mortality of males at all ages, the true number ought probably to be near 3.15.

TABLE, *exhibiting the Materials from which the Law of Mortality of the aggregate Population of England and Wales has been deduced.*

Between Ages.	Living on 28th May, 1821.				Dying in 18 years, 1813-30.			
	In England and Wales.		In Six Large Towns.		In England and Wales.		In Six large Towns.	
	Males.	Females.	Males.	Females.	Males.	Females.	Males.	Females.
0 – 5	791,579	774,689	24,716	24,330	736,039	622,903	36,449	31,744
5—10	693,858	682,457	21,853	21,940	87,263	79,732	3,817	3,448
10—15	603,613	569,366	16,759	17,982	52,324	52,155	1,673	1,544
15—20	509,586	535,569	12,424	17,752	63,405	71,535	2,033	2,089
20—30	755,780	901,338	18,709	31,338	144,586	163,140	5,622	6,001
30—40	593,662	649,507	19,447	24,991	123,996	140,848	5,757	5,855
40—50	482,329	500,977	16,220	18,219	129,675	*129,939	5,880	5,430
50—60	342,204	352,160	10,152	11,913	142,843	132,918	5,545	5,317
60—70	231,509	249,184	5,770	7,439	*182,207	179,251	5,991	6,223
70—80	115,032	124,648	2,499	3,572	202,208	211,028	5,314	6,312
80—90	29,587	36,315	616	942	116,726	136,085	2,506	3,636
90—100	2,253	3,280	44	86	14,486	21,304	300	597
above 100	60	129	5	4	637	1,263	20	48
Ages specified	5,151,052	5,379,619	149,214	180,508	1,996,395	1,942,101	†80,907	78,244
Ages omitted	683,114	765,090	2,961	4,389	75,199	80,265	—	—
Total	5,834,166	6,144,709	152,175	184,897	2,071,594	2,022,366	77,954	74,883
Estimated omissions	160,000	—	—	—	288,930	281,000	15,591	14,977

* There is a small arithmetical error, amounting to 200, in each of these numbers, as given by Mr. Rickman.

† The number of deaths whose ages are specified are in excess, because of the deaths in the suburbs of some towns having been included.

TABLE, *exhibiting the Average Annual Deaths for every Hundred Persons living in each of Thirteen Gradations of Age, according to Nine distinct Observations.*

Between Ages.	England & Wales 18 Years.		England & Wales 7 Yrs., 1818-24.		Six Towns 18 Years.		Sweden, both Sexes.		Table of "Mean Mortality."	Belgium both Sexes, 1829.
	M.	F.	M.	F.	M.	F.	21 Years, 1755-75.	20 Years, 1776-95.		
0— 5	5.35	4.60	5.29	4.56	9.21	8.06	9.01	8.50	6.73	6.58
5— 10	.72	.67	.70	.65	1.09	.97	1.42	1 36	.99	.87
10— 15	.50	.52	.49	.53	.62	.53	.66	.61	.65	.54
15— 20	.72	.76	.69	.76	1.02	.73	.76	.70	.75	.66
20— 30	1.01	1.04	.96	1.01	1.88	1.18	.92	.89	.93	.91
30— 40	1.14	1.24	1.09	1.21	1.85	1.45	1.22	1.16	1.25	1.00
40— 50	1.49	1.49	1.44	1.48	2.26	1.84	1.74	1.61	1.68	1.36
50— 60	2.34	2.16	2.26	2.08	3 41	2.76	2.64	2.39	2.40	2.17
60— 70	4.53	4.12	4.40	4.02	6.48	5.17	4,81	4.93	4.83	3.85
70— 80	10.12	9.69	9.95	9.49	13.28	10.92	10.23	10.41	10.04	9.09
80— 90	22.71	21.46	22.11	21.27	25.40	23.84	20.78	19.74	20.18	17.88
90—100	37.01	37.19	36.50	36.78	42.57	42.88	39.41	35.13	39.85	30.47
Above 100	61.11	56.06	68.06	58.61	24.97	74 12	—	—	70.00	—
All Ages.	2.17	2.07	2.12	2.03	3.39	2.68	2.89	2.68	—	2.27

The apparent mortality of males in all England and Wales, has been diminished by .09 between 20 and 30, and by .06 between 30 and 60 years of age, on account of the unenumerated maritime and military population. In the six large towns a greater correction is necessary, but none has been applied.

It might be supposed probable that the mortality deduced for females between the ages of fifteen and fifty years, may be subject to error from the disposition of females to understate their ages. The error is apparently of very small amount. If the ages of all the females, dying as well as living, be understated to the amount of two years, the mortality at each of the three decennial intervals from twenty to fifty years will be five per cent. higher than the fact. But if the ages of the dying are correctly given, and the ages of the living only are understated, then the apparent mortality between thirty and forty will be correct; whilst the mortality between twenty and thirty will be lower, and the mortality between forty and fifty higher than the truth. Adopting the intermediate supposition that the ages of the dying are understated to only one half the amount that the ages of the living are, the female mortality under the age of forty will be very nearly correct.

It has been already remarked, that the numbers representing the absolute mortality at each age in my present tables, are founded upon the assumption that the registered deaths are deficient twelve per cent. from the truth, and that Mr. Rickman has estimated this deficiency at eight per cent. The estimate of Mr. Rickman is not founded upon any fact, and leads to results contradictory of facts which he admits. My estimate is founded upon a probable fact, and leads to results in harmony with all other stated facts. Mr. Rickman agrees with me in supposing that the increase of the English population is entirely due to the excess of births over deaths, or that the immigration from Ireland compensates the emigration from England. Assuming this as a fact, the truth of my estimate and the error of Mr. Rickman's are easily demonstrable by means of the proportion stated to exist between the unregistered births and deaths.

In the population inquiry, questions were addressed to each clergyman requiring an estimate of the number of births and deaths in his parish unentered in the register. A portion of the clergy made such an estimate, another portion offered no estimate. The absolute number of "unentered" births and deaths of which there exists an estimate is evidently of very little value, because we are ignorant of the proportion of the total population from which this estimate was obtained. It may, however, fairly be presumed that the *proportion* of unentered births to unentered deaths would have been the same for the whole, as for this part of the population. This proportion is to be regarded as a valuable fact. For on the supposition that the increase of the population is due entirely to the excess of births over deaths, the difference between the un-

entered births and deaths is a known quantity, and the absolute number of unentered births or deaths is known when this proportion is known. Mr. Rickman's estimated deficiency in the deaths is, however, derived from the apparently groundless assumption, that the total deficiency in the deaths was just double the number of which an estimate was returned. Because the partial estimate amounted to four per cent. on the total, he has assumed that if all had made an estimate, the deficiency would have been eight per cent. He then shows, as a consequence of this assumed deficiency, that the unentered births were to the unentered deaths in the proportion of four to one. He entirely disregards the fact elsewhere stated, that the unentered births were to the unentered deaths as two to one, which proportion would have indicated a defect of twenty per cent. in the deaths, as I have shown in No. 614 of THE LANCET.

For the whole of England and Wales the partial estimate of the different clergymen indicates that the number of unentered births was just twice as great as the number of unentered deaths. If London be excluded, the proportion between the unentered births and deaths was that of three to one. This last proportion I have adopted as true for the whole of England and Wales, because it is applicable to nine tenths of the total population, and because the estimate for London is not much to be relied upon. The determination of the absolute mortality at all ages being a question of great public interest, it may be useful to exhibit the chain of facts upon which my estimated deficiency in the deaths is founded. I have confined myself to the observations on the female sex, because the increase of the female population is capable of being more correctly determined than that of the male population.

The total number of registered births of females in England and Wales during the eighteen years 1813-30 was 3,129,368, and of deaths, 2,022,366, which yields an excess of births over deaths of 1,107,002, instead of 1,658,202, the true increase of the female population in that time. There remains, therefore, the number 551,200 to be accounted for by the excess of unentered births over unentered deaths.

The total number of "unentered" obtained from those parishes which made an estimate, amounted to 357,168 births and 170,930 deaths of both sexes. For London, the estimate was 40,498 births and 65,934 deaths. Consequently for all England and Wales, excluding London, the partial estimate was 316,670 births and 104,996 deaths; which numbers are very nearly in the proportion of *three to one*.

If, then, it be assumed that the true deficiency in the deaths of females for all England and Wales amounted to 275,600, then three times this number, or 826,800, will represent the corresponding deficiency in the births. The difference between these unentered births and deaths is 551,200, the increase of the population to be accounted for as stated above.

The true number of births of females during the eighteen years will then be 3,956,168, and the true number of deaths 2,297,966. The female population alive at the middle of the eighteen years, or at the end of the year 1821, was 6,198,200. Consequently the average annual deaths of females was 2.06 per cent., or one out of 48½. The average annual births of females was 3.55 per cent., or one out of 28. The deficiency in the births amounted to 21 per cent., and the deficiency in the deaths to 12 per cent. According to Mr. Rickman, the deficiency in the births was 19 per cent., and in the deaths 8 per cent. According to my estimate the proportion existing between the total births and deaths of females is that of 172 to 100: according to Mr. Rickman the proportion is that of 175 to 100.

The population returns of 1831 abound with contradictory statements made by Mr. Rickman, founded upon the obviously false hypothesis of the population having been "stationary," and that the annual births had been equal to the annual deaths for a long period of time. Mr. Rickman does not appear to have thought that there was any absurdity in supposing 175 to be equal to 100. At a late hour, and since the publication of the returns, he has been made sensible of his error. In the pages of a medical periodical he has recently bestowed a great deal of unnecessary pains on the demonstration of his own errors. He appears to claim praise for detecting errors which could never have been committed by one possessing any knowledge of the subject discussed. He is greatly mistaken if he supposes that any writer of repute has said a word to the effect that the "hypothesis of the population being stationary," was applicable to circumstances like those of the English population. One specimen of Mr. Rickman's conclusions will be sufficient to satisfy the reader as to their value. Because out of 100 deaths of females in England and Wales at all ages 32 occur below the age of five years, he has stated as a fact that out of 100 *born*, 32 die before they complete their fifth year. The true statement is, according to his own showing, that 32 die out of 175 born, which represents a mortality of 18¼ instead of 32 per cent.

I have considered it unnecessary to construct a table rigorously representing at annual intervals the law of mortality which occurred in England during the period of observation. Such a table would be of very little value, because it would differ in an insignificant degree from a theoretical table

which I published more than three years ago. Moreover, there is reason to believe that this theoretical table being founded upon general principles, will be a better indication of future facts, than a table exactly representing one fact out of a great series of facts. The mortality of the female population of England is represented as near the truth as can be desired for any useful purpose, by my table of "Mean Mortality," when the limiting age of the period of "infancy" is taken at seven years. In the published table I have fixed this limit at eight years, which is the true limit for the male population of England. To adapt this table to the female population no alteration has been made except at ages under eight years. The results of this altered table agree very nearly in three important points with the results of the English observations for females. According to the table and according to the fact, the mortality between the ages of five and ten years is 20 per cent. greater than the mortality between the ages of ten and fifteen years. According to the table, there die annually 4.47 out of every 100 constantly living under the age of five years. According to the fact, when the registered deaths are assumed to be deficient 12½ per cent., the mortality of females under the age of five years was 4.60 per cent. According to the table, out of 100 born 19.5 die before they complete their fifth year of age. According to the fact when the registered births are assumed to be deficient 21 per cent., out of 100 born, 19 die before they complete their fifth year. The near coincidence of these results is satisfactory evidence of the general consistency of the materials, and of the conclusions deduced from them. It may be useful to state that the "expectation" or mean duration of life, at birth is 43.70 years according to the table of Mean Mortality, when the limit of "infancy" is fixed at seven years. If the English female population had been stationary, or if the annual births had been equal to the annual deaths for a long period of time, there would have died annually at all ages during the eighteen years 1813-30, one out of every 43.70 living. But on account of the increase of population, the actual annual mortality was only one out of 48¼.

On the supposition that the registered deaths are deficient 12½ per cent., the mortality of the English population between the ages of sixty and seventy, and between seventy and eighty years of age, agrees very nearly with that of the table of Mean Mortality. According to this table, 204 living between the ages of sixty and seventy are reduced by deaths in ten years to 100 living between the ages of seventy and eighty years. According to the fact stated in the returns of 1821, there were of both sexes 201 living between sixty and seventy for every 100 living between seventy and eighty years. If the population had been stationary between sixty and eighty years, the coincidence of these numbers would prove that the absolute mortality at this interval has been truly stated, and is represented by the Table of Mean Mortality. If the absolute mortality is correctly stated at any one interval, it is correct at every interval of age, because the scale of relation connecting together the mortality at different ages is indisputably established. That the population was stationary at the period in question, or that the living in 1821 between the ages of sixty and seventy proceeded from the same number of births as the living between seventy and eighty, appears to be highly probable. At least this was certainly the case in London, and there is no ground for supposing that the rest of England did not resemble London in this respect. Those living in 1821, between the ages of seventy and eighty years, were born in the ten years 1741-50, and those living between the ages of sixty and seventy were born in the ten years 1751-60. Now, according to the London Bills of Mortality, during the ten years 1730-39, the number of baptisms amounted to 170 thousand. In the ten years 1740-49 they amounted to 146 thousand; in the ten years 1750-59 they were 148 thousand; and in the two following decennial intervals the baptisms amounted to 160 and 173 thousand respectively. It may hence be inferred as highly probable that the English population in 1821 was decreasing between the ages of eighty and ninety years, stationary between sixty and eighty, and increasing under the age of sixty years.

The new theory of mortality is founded upon the discovery of three numbers, respectively presiding over three well-marked periods of human life—before, during, and after, the existence of the procreative power. To construct a theoretical table, the absolute mortality at any one age must be known, together with the position of the inferior and superior limits of the "procreative period." In every population, whatever may be the absolute mortality, the relative mortality at different ages is the same, when the position of these two limits is the same. In the Table of Mean Mortality I have fixed the superior limit at the age of fifty-five years, which happens to be the true limit in each of the nine independent observations above stated. In all these observations, and in every county of England, the mortality between fifty and sixty is to the mortality between forty and fifty years, in the proportion of three to two, as it is also in the Table of Mean Mortality. Notwithstanding this extensive coincidence, the position of this limit is not to be regarded as having more permanency than the position of the inferior limit. A variation of one year in the position of the superior limit involves a change of five per cent. in the rela-

tive mortality; the same variation in the position of the inferior limit involves a change of 32 per cent. in the relative mortality. In constructing the Table of Mean Mortality, I ventured, in opposition to all direct evidence on the subject, to put back the inferior limit one year, and thus effected a diminution of 32 per cent. in the relative mortality under the age of nine years. My prediction has been confirmed, and more than confirmed three years after its publication, by the present observation on the English population. According to all previous observations, the inferior limit, or the mean age of attaining the minimum mortality, was at nine years—the mortality between five and ten years being always twice as great as the mortality between ten and fifteen years of age (as it is now in the large towns of England), which agrees with theoretical tables wherein the inferior limit is fixed at nine years. In the table of "Mean Mortality," I assumed the inferior limit to be at the age of eight years, which is the limit now applicable to the total male population of England; the mortality between five and ten years being 50 per cent. greater than the mortality between ten and fifteen, according to fact and according to the table. But for the female population the limit is at the age of seven years, for the mortality between five and ten is only 20 per cent. greater than the mortality between ten and fifteen, according to fact and according to a table wherein the age of seven years is assumed as the limit. In Belgium the position of this limit is now at the age of eight years and a quarter for both sexes. The grounds of my accomplished prediction were the following:—From the ages of the living under fifteen years, and from the rate of increase in the births, indicated by the population returns of 1821, I became convinced of the fact, that the mortality under the age of ten years was less in an extraordinary degree than had ever been supposed to exist. I knew also that the diminution in the mortality during infancy was amply sufficient to account for the reduced mortality in England at all ages. Knowing then that the mortality was considerably diminished under the age of ten years, and not diminished above that age, I accounted for it in the only way consistent with the new theory. The truth of this theory has now been confirmed by the highest order of human evidence, for it has enabled me to predict the precise manner in which a new and extraordinary diminution of the relative mortality in infancy has been effected.

In every county of England the proportion of deaths occurring at annual intervals between the ages of one and six years, agrees with what I have stated to be the universal law of mortality. The deaths in any two consecutive years of age are always in the proportion of three to two, according to fact and according to theory, as may be perceived by any person without calculation. Under the age of one year, the theory is correctly applicable to the county of Cornwall only. In all other counties, the proportion of deaths under the age of one year is much greater than that indicated by the theory. Four years ago, in the preliminary observations to my "LIFE TABLES," I expressed an opinion that the theory would not represent the fact in all cases under the age of eight weeks. I believed then, as I believe now, that the high mortality under the age of one year will be found to consist in an excessively high mortality during the few weeks immediately succeeding birth.

As the subject which I am discussing naturally divides itself into two parts at the point now attained, I shall here arrest my remarks until the publication of another Number of THE LANCET will admit them to appear.

46, Regent Square, Nov. 7, 1835.

CASE OF

GANGRÆNA SENILIS,

IN WHICH THE POST-MORTEM EXAMINATION DISCLOSED A

COAGULUM OF FIBRINE ADHERENT TO THE AORTA.

To the Editor of THE LANCET.

SIR,—If you consider the outlines of the following case worthy of being recorded in your truly independant journal, I shall feel obliged by its insertion. It is, in my opinion, calculated, in some degree, to illustrate the pathology of a disease which is too frequently fatal.—I am, Sir, your obedient servant,

W. TAGERT,
Surgeon to Mercer's Hospital,
Dublin.

CASE.—Catherine Strahan, aged 66, of a delicate and feeble frame, was admitted a patient into *Mercer's Hospital*, on the 25th of September, 1835. She then suffered from distressing diarrhœa, and complained much of pain in the left leg and foot, accompanied by a constant sense of coldness. This coldness, and the pain of the extremity, with occasional diarrhœa, afflicted her about a month previous to her admission. The pain was so urgent as to interrupt her sleep. She attributed her sufferings to rheumatism, and kept the limb wrapped in flannel. She did not suffer from pains elsewhere, but her general health was broken. On admission, the leg and foot presented a natural appearance, and she stated, that when suffering from cold, the limb assumed a livid hue. The diarrhœa was relieved by the ordinary

treatment, but the pain and uneasiness of the limb continued unabated, and became daily more urgent. On the 1st of October, almost a week after her admission, a darkish livid hue was observable on the integuments of the left instep, which felt colder to the touch than the opposite one. This lividity soon extended to the outer ankle, and, after two or three days, involved the entire foot. At this period there was no sign of vesication about the toes. On examining the femoral artery in the groin, a slight thrill could be distinguished. Its pulsation was evidently weaker than was the pulsation in the opposite limb. The pain was now very severe, with nocturnal exacerbations. The extremity was wrapped in cotton, and oiled silk was placed outside this, and anodynes were administered to alleviate her sufferings. The lividity extended up the limb, day after day, and vesications soon formed on the leg and foot, from which there was serous oozing. The gangrenous appearance now occupied the entire limb, almost to the knee, but evinced no disposition to extend higher, for about ten days previous to her death. I may observe, however, that an isolated gangrenous spot appeared over the patella, and in three days afterwards the thermometer indicated the following temperature :—

Heat of the ward, 68°;
Heat of the sound limb, 82°;
Heat of the gangrened limb, 74°.

At this period no pulsation could be distinguished in the femoral artery at the groin.

It is unnecessary to give a minute detail of the daily treatment pursued. It will be sufficient to observe, that it consisted principally of the internal exhibition of the acetate of morphine, with a cautious administration of wine and stimulants, combined with tonics, the extreme debility and unceasing pain being the prominent indications. She sank gradually under her sufferings, which terminated in death on the 22nd of October, nearly one month from the period of her admission into the hospital. The diarrhœa had returned, and continued unchecked for a few days previous to her decease, notwithstanding the use of the cretaceous mixture, with kino, opium, and other astringents.

Autopsy, ten hours after Death.

Nothing remarkable was discernible either in the thoracic or in the abdominal viscera. In order to examine the large arterial trunks, I had the aorta carefully removed, together with its continuation into the common iliacs, and also the entire of the main arterial trunk of the diseased extremity, with a portion of the internal and external iliacs of the opposite side. On slitting up the aorta, its coats appeared to be healthy in the thoracic region; the internal coat of the abdominal aorta presented a number of whitish spots, of an irregular figure, which were slightly elevated, containing an earthy or osseous deposit. This atheromatous deposition was external to the lining membrane, and unaccompanied by any inflammatory appearance. The aorta at its bifurcation was occupied by a coagulum of fibrine, of a conical shape, three inches in length. The base of the cone was turned from the heart; its colour was a yellowish white, without a trace of red colouring matter; it was firm in consistence, resisted strong pressure with the finger, and gave to the vessel a solid fleshy feel before it was slit open. It was intimately adherent by its base to the entire circumference of the artery, and could not be removed without tearing the bond of union. The organization of the coagulum was so perfect, as fully to demonstrate that it had existed during life, and had been possessed of vitality. The apex of the coagulum was also adherent to the aorta. Its base, for the extent of more than an inch, blocked up that vessel, and, immediately above its bifurcation, filled the cavity of the aorta so completely as to act as a perfect barrier against the transmission of blood through that portion of the vessel. Coagula of a similar appearance, but not adherent, occupied both of the common and internal iliacs. The external iliac of the right side was healthy, and unoccupied by coagulum. That of the opposite side contained a fibrous plug. The femoral artery of the diseased side was much contracted in its entire course, even to the ham, and was occupied by coagula of a reddish-brown colour, but of firm consistence. High up in the groin, near Poupart's ligament, the coagulum was intimately adherent to the artery. The popliteal was much contracted, and the posterior tibial was so diminished in size, that it could with difficulty be recognised; it contained no coagulum; the muscles of the gangrened limb were much softened, and were dark in colour. The aorta, and the remainder of the arterial trunk, with the coagula *in situ*, I have preserved in the museum at *Mercer's Hospital*.

Remarks.—With regard to the question, Was the coagulum of fibrine found in the aorta a post-mortem deposit, or did it exist during life? I have to state, that, independently of any reference to the history or symptoms of the case, I think its existence during life is fully proved by the fact of its organized connection with the artery. It may also be asked, Were the coagula the cause or the effect of the gangrene? Upon this point I shall refer to the following extract from "*Andral's Pathological Anatomy.*" After speaking of the various ways in which an artery may become obliterated, Andral makes the following remark: "The second

species of obliteration (that is by coagula of fibrine) has been more frequently observed than the first, *but no instance of it has as yet been seen in the aorta;* it has been repeatedly found in the arteries of the lower extremities, coinciding with the disease known by the name of gangræna senilis, and it is reasonable to conclude that in these cases the obliteration of the arteries is the cause of the gangrene. This much at least is certain, that it is not *the effect;* for in a patient who died of gangrene in one foot, and in whom all the arteries of that limb were completely obstructed by solid coagula of fibrine, the commencement of a similar obstruction was found in the arteries of the opposite limb, although not a sign of gangrene was observable in it. It appears to me highly probable that if this individual had lived for some time longer, the arteries of the sound limb would have become more and more obstructed, and that the foot of that side would then have become gangrenous."

The following passage from Dr. Thomson's excellent work on inflammation may not be considered out of place here. In the article "Mortification," he says, "Fabricius Hildanus mentions a case of mortification of the feet and legs which proved fatal to a man in the vigour of life, and of apparently sound constitution, in whom after decease a scirrhous tumour was found surrounding and compressing the inferior cava and aorta, just where they are about to divide, to form the iliac vessels, and which by its pressure had prevented the free passage of the blood to and from the lower extremities."

Beclard also was of opinion that the obliteration of the arteries was the *cause* of the gangræna senilis, as will appear from the following extract from his "Elements of General Anatomy:"—"Contraction and obliteration of the crural trunk and its branches. This is the ordinary cause of the gangrene of the toes, feet, and legs, in old people, the change happening in a part, and at a period, when the arterial twigs themselves, affected by induration, are no longer capable of the rapid augmentation necessary for the establishment of a collateral circulation."

I shall not trespass further on your valuable space than just to observe that I lately witnessed the dissection of another female who died of the same disease, and in whom there were found most extensive ossific deposits in the thoracic and abdominal aorta, as well as in the artery of the affected limbs. In this case also the posterior tibial artery was so diminished as to be found with difficulty.

EXCESS OF

FATTY MATTER IN THE BLOOD.

DEFICIENCY OF THE RECTUM.

To the Editor of THE LANCET.

SIR,—I send you two cases of unusual occurrence, the first recommended from the remarkable milky appearance of the blood; the second being a case of imperforate anus, with deficiency of the rectum. By giving them insertion in your valuable journal, you will oblige your obedient servant,

THOS. ROBT. TATHAM, *Surgeon.*

Huddersfield, Nov. 27, 1835.

CASE.—April 25th, 1833.—Mr. William Bond, ætat. 36, publican, of a florid complexion, rather corpulent, and of a sanguineous temperament. I found him suffering from a severe catching pain in the right hypochondrium, much aggravated upon pressure, on the slightest attempt to take a deep inspiration, and also increased by the recumbent posture. He had headache, the countenance was flushed, and there was a suffused redness of the eyes, tongue furred, dry in the centre, much thirst, surface hot and dry. Pulse 136, full and hard; urine high-coloured; bowels confined. Says he has not been quite well for the last fortnight, having suffered from loss of appetite, disagreeable taste, and "greasy" eructations; did not vomit; complains that his vocation exposes him more to the influence of liquor than is consistent with health.

V. S. ad deliquium animi. Thirty ounces were taken.

R. *Calomel.* gr. xij; *Pulv. Opii* gr. iij. Cons. q. s. Divide in pilulas vj. Capt. 3tiis horis.

R. *Mistur Effervescentiæ*, sumend. 3tiis horis.

P.m. Pain in the side much relieved, breathes with tolerable ease, can lie down in bed without inconvenience, but cannot take a deep inspiration without pain; skin become moist; pulse 130, softer. The blood as it stands, undisturbed, appears like thick custard; the coagulable lymph on the crassamentum is a quarter of an inch thick, and the serum has precisely the appearance of milk. These peculiarities induced me to submit it to the inspection of Dr. Walker and Dr. Turnbull, physicians, of this town. Dr. Walker visited the patient with me on the following day. Upon drawing off the serum from three teacupfuls of this blood, leaving the crassamentum behind, and mixing with it half its volume of sulphuric ether, it yielded, upon evaporation, upwards of two drachms, in weight, of fatty matter, of the consistence of butter.

26. Improving; pulse, 126; lateritious sediment in the urine; bowels have not yet been opened. ℞ *Ol. Ricini* ʒvj, statim sumend. et rep. 6tis horis, si opus sit.

27. Two doses of the oil produced three dark-coloured and offensive evacuations.

May 3. Convalescent. During the last few days more active cathartics were administered, which have restored the healthy appearance of the evacuations.

Case.—January 16, 1835.—I was sent for by Mrs. Whitworth to examine her male infant, born on the 14th inst., which she had just discovered had an imperforate anus. The infant had been indisposed from its birth, and refuses the breast and all nourishment. It is very restless, almost constantly crying, holds itself stiff, turns blueish, and appears to be threatened with convulsive fits. She attributed these symptoms to uneasiness in its bowels from its not having had an alvine evacuation. The greenish slimy appearance of its urine upon the cloths, led her to make an examination, when she discovered an absence of the natural outlet from the bowels. The raphe, from the scrotum, is continuous through the perineum. There is no vestige of an anus, no doughy feel, nor any sense of fluctuation on making pressure over the parts. Mr. Robinson, surgeon to the *Huddersfield Infirmary*, accompanied me in the afternoon to see the patient, when we decided upon an operation without further delay. I commenced by making an incision in the line of the raphe a little below the scrotum, and carried it rather beyond what I supposed to be the proper situation of the anus. This incision might be an inch and a half in length. I then carefully dissected backwards, to avoid wounding the urethra and bladder, and downwards, towards the sacrum, full one inch from the surface of the perineum, before I reached the gut, which was much distended, the infant forcing very much the whole time. I made a free cut into it, which was followed by a very copious discharge of meconium. I then passed with the greatest ease, and without apparent increase of pain to the infant, my left forefinger into the colon. Oiled lint was placed in the opening. Very little blood was lost in the operation, but blood oozed much during the night. The child, however, continued hearty, and partook freely several times of arrow-root.

February 1st. Doing well. Fed with the spoon; the mother could not get it to suck, and her milk has now disappeared. A little oiled lint, and simple ointment on lint, were used during the first five or six days. Afterwards I made a bougie of tolerable size, and covered it with wax, which is introduced three and four times a-day, allowing it to remain for a quarter of an hour or twenty minutes each time.

10. Up to this day the infant continued to do well; it has made water in natural quantity, quite clear from fecal matter, and passed its stools without much inconvenience. The use of the bougie has been diligently persevered in three and four times a-day; nevertheless the parts have contracted rapidly. Passed per urethram this morning a quantity of fecal matter. Phymosis has come on; the scrotum is slightly œdematous; and the perineum is inflamed, swelled, and painful. Bowels rather confined; dejections of a pipe-clay appearance. In consequence of the inflamed state of the parts I desired them to desist from using the bougie for a day or two, and ordered a zinc lotion to be applied to the affected parts, and a dose of castor-oil, and some alterative powders to be taken twice a-day.

13. Much the same; the oil operated freely, takes scarcely any nourishment, very restless; there is now much purulent discharge from the urethra.

15. Takes more nourishment; parts less inflamed; bowels more regular; the bougie is again used, a little bloody discharge follows each introduction.

19. Rests better and takes more food; yet is weaker, and more attenuated; the urethral discharge continues not so severe; the phymosis and œdema of the scrotum have subsided. Dr. Walker and Mr. Robinson called this morning, but suggested nothing that could be of decided relief. The mother, of her own accord, has given it, during the last few days, a little Godfrey's Cordial.

26. Much the same; discharge of fecal matter, *per urethram*, continues daily; scrotum very much relaxed; screams violently at every attempt to make water; the introduction of the bougie causes spasm of the bladder, and a flow of urine,—bloody mucopurulent discharge following its removal.

March 15. Continues to decline; is much more attenuated; the appetite is variable, sometimes very voracious; the artificial opening is more contracted; the other symptoms are as before noted.

20. Dead.

Autopsy on the 21st; Dr. Walker and Mr. Robinson present.—Body very much emaciated; thoracic viscera healthy. On opening the abdomen, the omentum was found nearly free from fat; the large bowels were greatly distended with air; liver much shrunk, small intestines of a shrivelled appearance; stomach and mesentery natural. I detached the scrotum and penis, with the corresponding integuments, from the pubes, carefully divided the symphysis, and separated the bladder from its connection with the inner surface of those bones. Passing a director through the urethra into the bladder, and slitting it open superiorly, I found the opening to be a small narrow canal, a quarter of an inch in length, situated immediately behind the prostatic part of the bladder, communicating with the broad and sacculated

extremity of the colon. The gut contained a little bloody muco-purulent matter, and curdled feces; it had descended to within half an inch of the surface of the perineum. This portion, judging from the appearances, would soon have been in a highly diseased state.

MODIFIED SMALL-POX

OCCURRING

TWENTY-TWO YEARS AFTER VACCINATION.

Turning on the third day after the appearance of the Eruption.

To the Editor of THE LANCET.

SIR,—I beg to send you the following case for publication.—

J. S., ætat. 51, of short stature, and well formed, possessing general good health, and having scarcely ever known, as he states, "what it is to be ill," was seized with a sharp attack of fever, attended with severe headache, which he attributed to cold. A dose of calomel, and an aperient, relieved him, but he was unable to quit his bed, on account of debility and giddiness, for ten days, during which period there was occasional remission of the febrile symptoms, which on the sixth day assumed the intermittent form. On the eighth day, *two* incipient pustules appeared on the hand, succeeded by others on the face and body. I at first imagined these to be varicella, but the number and character of the pustules on the tenth day were unequivocal. The eruptive fever was severe; about a dozen pustules arrived at maturity; those on the hand progressed regularly, but on the face and some parts of the body they died away before the formation of pus. On the tenth day, those which had maturated became flattened, and were scabbed on the thirteenth. The patient gradually recovered, and was quite well on the fifteenth day.

On examination of the arm, there appeared two well-defined cicatrices. The vaccination had been perfect, and the patient described it as "a very fine arm." The rise and progress of the pustules were *steady and uninterrupted throughout. No lymph was taken from them*, and to this fact I am disposed to attribute his preservation from the disease, which, but for this ægis, would, in all probability, at his age, have terminated fatally.

I am, Sir, your obedient servant,

T. W. WANSBROUGH.

Fulham, Nov. 18, 1835.

DOUBTFUL SEX.

CANCER OF THE RECTUM SUCCESSFULLY REMOVED.

To the Editor of THE LANCET.

SIR,—I take the opportunity of a friend proceeding to London, to send you two cases which you may consider of sufficient interest for publication.

I am, Sir, your obedient servant,

D. R. RANKIN.

Carluke, Lanarkshire, Nov. 15, 1835.

CASE 1.—A case of that peculiar conformation which is usually distinguished, however inaccurately, by the term "Hermaphrodite," has lately fallen under my immediate observation, and as it is remarkable in some degree, I shall in the following short description try to bring before the reader as distinct a picture of the case as possible.

The child is about ten months old, healthy in every respect, and well made. The present appearance of the infant, so far as regards the external parts of generation, is much the same in proportion as it was at birth. At certain times, without close inspection, the parts present all the characteristics of the female; the mons veneris is freely rounded, and the labia majora are of the usual dimensions, in the commissure of which the clitoris is merely seen. At other times an opposite appearance is presented; the labia disappear, a scrotum hangs loosely down, and, in place of the clitoris, a penis, small in size, is beautifully displayed.

During the first-described state of the parts, on more minute examination, the labia are found to be covered with integument, corrugated, and in other respects like that which forms the scrotum, and to contain, each, a small moveable oval body, like a testicle. There are no nymphæ; but a fold of integument, reflected from that protuberance which occupies the place of the clitoris, simulates faintly those processes. The body holding the place of the clitoris, has a mucous membrane for its covering, is cylindrical, about three-fourths of an inch long, rather disproportioned in thickness, not unlike, in shape and size, to a large well-formed female nipple, and very much resembles a penis. The glans with its perforation, and the prepuce (short and incapable of covering the glans) with its frænum, are most distinct; but there is no corona glandis, or cervix. Beneath, and at the root of this body, there is a small orifice, which proves to be the proper urethra; and under this, indeed in its proper situation, another opening, the vagina, which, in appearance and depth, is of the normal standard.

In the other state of the parts described,

while the female peculiarities are sufficiently well marked, the male organs are very fairly represented, only the scrotum is divided in the middle, giving a pouch to each testicle, and the penis is short.

The anatomical structure of the penis, or clitoris, in this case, it is not easy to determine; it is, however, an erectile body,—a characteristic of both. While in the erect state, it can be very easily traced to its root, which seems to be over the symphysis pubis —there being no trace of it in the perineum.

The question, Is it male or female? has a hundred times been asked; a question which, in the meantime, cannot be decided positively. Independently of any other consideration, the presence of testicles, with some, decides the matter; but the minute inquirer perhaps will not rest satisfied with such evidence, so long as the functional powers are questionable. May not there be a womb, and appendages, and ovaries too? A near approximation to such a formation has been proved oftener than once, and we do not know what may be the result of further investigation.

Case 2.— Mrs. ——, aged 55, of weakly constitution, was first seen by me towards the end of Nov. 1834 She had for nine months endured a great degree of torture, from pain in the rectum; for the last six weeks she had had discharge from the anus, of very offensive matter, and the pain was now so incessant and severe, that life had become a burden. She had been attended, during the period above-stated, by the family surgeon, who supposed that it was a hemorrhoidal affection from which his patient suffered, and treated her accordingly. He had even lately "cut away a blind pile," as he called it, namely, a part of the scirrhus.

On examination per vaginam, a flat, indurated, inelastic body was distinguishable on its posterior aspect, and on introducing a finger into the rectum, and passing it on the anterior aspect, an extensive, irregular, tubercular surface was detected. With the aid of the speculum the whole was brought into view, and the nature and extent of the disease at once seen. The diseased mass, a congeries of tubercles of various dimensions, extended nearly four inches along the anterior of the rectum. It was about two inches broad; the margins were well defined, and the rest of the bowel seemed to possess its integrity. As the only probable means of relieving the immediate sufferings of the patient, and the only hope, however small, of affording permanent relief, or at least of prolonging life, an operation was recommended, Mr. Logan, of Lanark, concurring in the proposal, to which the poor sufferer willingly responded.

On the 29th of Nov., after a renewed and careful examination of the parts, the operation was performed. The recto-vaginal septum was nearly two inches thick at the perineum, and, at the superior part of the tumour fully an inch; the coat of the vagina, posteriorly, were greatly attenuated, and proved, at some particular points, to be involved in the morbid mass. It was therefore thought vain to endeavour to preserve it. The patient being placed in a suitable position, the speculum was introduced into the rectum, and so managed that one of the arms ran along the anterior margin of the diseased intestine on the right side, which was the line of the first incision. The knife was passed along the arm of the speculum, while the index-finger of the left hand was introduced into the vagina, to receive, at the highest part of the tumour, the point of the knife, which, guided in this way, was brought freely out at the perineum. The part thus freed on one side, was grasped by an assistant with a long flat forceps, and carried to the right side. The incision on the left side was then made in the same way. The whole diseased portion was now taken hold of by the left hand, and pulled down until the knife easily reached beyond the disease, which was detached by a transverse incision. There was little blood lost, and the cautery was required to be applied only at two points. A sponge, loaded with a weak solution of nitrate of silver, was introduced into the cavity, and the patient put to bed. Setting aside the inconvenience experienced by the involuntary discharge of feces, and the falling down of the womb, no untoward symptoms ever appeared, and the patient now enjoys comparatively good health.

The diseased portion of the rectum, on inspection, presented all the characteristics of tubercular scirrhus. Ulceration had commenced in three different places, and there was one ulcerated cavity large enough to admit a walnut.

EARLY BREEDING IN THE HEIFER.

P.S. I have notes of a few interesting cases, (?) not strictly medical however, which have lately occurred (?) in my neighbourhood. One of an animalcule in the eye of a horse. One of singular monstrosity in the shape of a chicken with *four* legs and *three* wings. And a case of precocious impregnation in the cow species. A bull calf of *less than three months age*, and a quey calf of about *two months*, copulated, and within nine months the female brought forth her first-born; both mother and offspring being yet alive. Will details of any of these phenomena suit the pages of The Lancet? If so they shall be forwarded.

*** We find the following statement, by Mr. W. A. Cartwright, of Whitchurch, in the *Veterinarian* for November, 1835, bearing on the subject of early œstrum and impregnation in the heifer:—

"A Mr. Wilson, a respectable farmer, residing at Alkington Hall, had a cow that calved her third calf in January, 1834. When about six months old, this calf was bulled by a bull calf of the *same age*, when running out with her. In about nine months after she calved very well, and brought a heifer calf, which is now rearing. It is rather small, but healthy. The breed of this heifer is of the short-horn."

In the same periodical for December, the Rev. Henry Berry, alluding to the above occurrence, makes the following statement. The subject is interesting to physiologists:—

"I this year lost an improved short-horned heifer, from the injury experienced in extracting a calf at a period when the dam was *only thirteen months old*. The bull's access to her at such an early period was of course accidental; but the circumstance of calves seeking the bull at this early age is quite common in my stock, most particularly if the calves be allowed to suck the dam. Strange though the circumstance may seem, I find them more susceptible of impregnation at a very early age than when more matured; and, still more strange, my late but frequent experience has been derived from cases where the calves, unmanageable under these circumstrnces, have obtained access to a *particularly large* bull, whose size would suggest the impossibility of consummation. I think it probable that *any* calf allowed to suck the dam, would come earlier in season than otherwise, but the improved short-horns are peculiarly characterized by *general* early maturity, and I should, even without the testimony of experience, be inclined to expect the result under consideration, as well as the other indications of such early maturity."

THE LANCET.

London, Saturday, December 5, 1835.

THE excitement in the profession on the subject of the medical contracts which are still being made in the new parochial unions, is on the increase. The dissatisfaction is deep and general, and sincerely do we hope that the indignation of the members of the profession will undergo no abatement, until the vile, the abominable, the brutal system on which the making of these contracts is founded, shall be for ever abolished.

In a recent number of this Journal we endeavoured to show that it was unjust to condemn the Poor-Law Commissioners, when those gentlemen had expressly declared to Mr. HODGES, M.P. for Kent, and other gentlemen, that the Boards of Guardians should exercise the unrestricted privilege of arranging the conditions of the medical contracts in the new parochial Unions. In offering those exculpatory remarks we relied on the word and promise of the Commissioners,—our reliance was, however, but indifferently founded, if the statements which are contained in the letter of Mr. CEELY, of Aylesbury (inserted at page 387 of this week's LANCET), be supported by a correct apprehension of the circumstances which he has explained. MR. CEELY does not write anonymously. He is a most respectable and intelligent practitioner, and we place, therefore, the fullest reliance on the details which he has communicated. It would now appear that the charge of interference in arranging the medical contracts between the boards of guardians and medical practitioners, is brought home distinctly and unequivocally to the Commissioners and their agents, and thus the promise which was so unreservedly made to the Kent deputation, has been unfeelingly and unblushingly falsified. It is really impossible to write or to speak on this perverse and cruel exercise of authority, with any degree of moderation or patience. The Commissioners and their agents are resolved, it seems, on reducing the amount of the poor's-rates. "No matter what may be "the consequence to the poor,—no matter "what sufferings may be inflicted,—no "matter what lives may be lost,—THE POOR-"RATES MUST AND SHALL BE REDUCED." Thus would speak the Poor-Law Commissioners. But a declaration of a very different character will be heard from the mass of the people of this country. The conduct of the Commissioners and their agents relative to the contracts, is bringing the Ministers of the Crown into disrepute and odium everywhere throughout the rural districts, and, Heaven knows, a reforming

ministry stand in need of no such depressing auxiliaries in those quarters.

There are, in England and Wales, upwards of *fifteen thousand* medical practitioners, whose occupations necessarily lead them into all ranks of society, and whose learning, usefulness, and integrity of character, render them the objects of universal esteem and respect. Leaving out of consideration, therefore, the protection of the poor, which ought to be the first subject of consideration, can any executive government be justified in suffering their officers to treat with disrespect, contumely, and insult, such a body as is constituted by the members of the medical profession? But we are firmly convinced that neither the Prime Minister nor the Secretary of State for the Home Department has sanctioned any one of the disparaging acts of the Poor-Law Commissioners relative to the medical contracts. When Lord JOHN RUSSELL was addressed on the subject in the House of Commons, he spoke in a tone of the most fervent earnestness, in deprecating any arrangement with medical practitioners, which could have the effect of derogating from the interests of those gentlemen, or of withholding from the poor any essential medical aid in the hour of sickness. The conduct, therefore, of the Poor-Law Commissioners and their assistants must be brought under the consideration of the executive Government, and of Parliament, and we will soon see whether the evil, great as it is, cannot be removed by an efficient and permanent remedy. The course of conduct which the Assistant-Commissioners are pursuing, is calculated to drive the people into a state of fury, of madness. If it be the object of those hired functionaries to *destroy* the poor sufferers, let them have the honesty, the boldness, the courage, to make the acknowledgment; but let them not, under the specious pretext of providing the poor with competent practitioners from a distance, send amongst the wretched and destitute sufferers, a set of VAMPIRES, who willingly consent to obtain a disgraceful livelihood, by a profuse sacrifice of human life. What offences have the poor of this country committed that they should be deprived of the services of respectable resident practitioners, and be turned over to a set of low-minded mercenary adventurers? Are the meritorious labouring men of England to share a worse fate, with respect to medical aid, than the criminals in our jails? Let us state one fact in order to exhibit the scandalous measure of injustice with which our poor are treated, and the opposite course which can be pursued in the case even of criminals, when *patronage* is to be bestowed. When the "estimates" were before the House of Commons in the last session of Parliament, votes of money were demanded by Ministers, for the supply of medical attendance on the prisoners who were confined in the Penitentiary of Milbank. The number of criminals was under 600,—all, observe, *at hand*,—confined within the walls of a single establishment,—and every prisoner necessarily rendered subservient to the orders of the medical attendant. The Penitentiary is not *an hospital*. There were not five hundred and odd *sick* persons placed there, but six hundred healthy persons, simply exposed to the *casualties* of disease. And what sum was voted by the House as the salary of the resident surgeon? Why, *three hundred pounds*, for one year! And what for the visiting surgeon? *Three hundred pounds* also! These sums, be it observed, were *exclusive* of the charge for *medicines*. Now observe. Medical aid is provided for *criminals*, in a state of *health*, at a charge, per head, of *upwards of one pound per annum*, while the poor labourers in the Unions are provided with the occasional presence of a medical adventurer, at a charge varying from *two shillings* to three shillings and sixpence per annum, and no more,—if he be sick, if he be bed-ridden,—*for a whole year*, and, further, if his miserable mud-hovel be ten miles from the abode of the merciless contractor!

Good God! We blush for the honour of our country, when we find atrocities of this description commonly and unhesitatingly perpetrated. It is impossible, utterly impossible, that Ministers can be guilty either of the cruelty or the folly of giving their sanction to such a brutal system. On this point, however, it has become necessary that the most accurate information should be at once obtained.

The circumstances connected with the filling up of the vacancy caused by the resignation of Mr. Kirby in the Dublin School, will be stated in the next Lancet in the communication of a correspondent. We have not space on the present occasion, to follow the intriguers through the windings of their crooked and despicable policy, our object being merely to place before the world a specimen of the corruption which forms the staple of a medical corporation. The details, of proceedings of this kind, are worse than imagination can in general conceive them to be. Let the reader assume any ideal standard of turpitude, and he will find it, on examination, to fall far short of the reality of the present proceedings, into which not one useful or equitable object enters. As to the qualifications of the candidates, or the principle of conferring the office on the most deserving competitor, the parties do not even pretend to think, much less to speak. That would be "radicalism" and "revolution." When the *concours* was proposed some time ago in this Institution, every objection was made to its introduction, on the grounds of its complexity, and the difficulty of carrying it into execution. "Such a plan of disposing of professorships," said the junto, "was perfectly impossible." But what mode of effecting such an object can equal in tumult and intricacy, the plan pursued in the present instance, where we have all the elements of discord, and the most circuitous of processes, along with the demoralizing effects of *such* contests on the Members of the College, and the perpetuation of the grossest injustice to the candidates?

The professor of the "Institutes of Medicine" in the School of Physic in Ireland has been practically illustrating of late the moral of the "little harper in the great theatre." Like the puny musician who mistook the acclamations of his village admirers for the voice of fame, and fancied that the tones of his diminutive lyre would be equally successful in securing the applause of the world upon a larger stage, he has been exhibiting himself, under the impression, no doubt, that the public would repeat the parasitic approbation of the coterie of medical conservatives in the Irish metropolis, and he seems to have had no friend to admonish him of the imprudence of the experiment—no Moliere's old woman to refrain from laughter during the perusal of his vagaries. The course he has adopted is, perhaps, the best calculated to undeceive his self-love, and bring his followers to their senses. It is possible that with his address he might retain their allegiance a little longer—might continue their "consulting physician" and "medico-political advocate" some few months more—were it not for his furnishing them so abundantly with the means of detection.

Let us see how the account stands between these trusty adherents and their disinterested leader. The compact was this,—for it would be idle not to presume a reciprocity of advantages between these honourable individuals—that their bigotry should be defended, and their monopoly secured, against the ravages of Reform. Upon one side were expected the brilliant efforts of an accomplished pleader, and the imposing authority of a man of science; on the other the patronage of pupils and of patients, and an annual subscription for "the Journal." But in what have these expectations eventuated? One of the Journals, selected as the mirror for reflecting abroad the beauties of medical science in Ireland, has died a natural death; the other has been preserved from a similar fate by a timely subscription during the recent summer. As refutations of the described condition of medical affairs in Ireland, we have had oracular contradictions;

and in science, the gleanings of continental authors put forward as productions of Irish growth. In physiology we have had fine specimens of philosophy run mad,—and in therapeutics, pleasant juleps and agreeable mixtures. In medical literature, the labours of a Grub-Street compiler,—and in materia medica, the commentaries of a confectioner!

By these Herculean exploits were the patronage, the tuition, and the practice, of the medical profession in Dublin, to be secured for ever. Considering the condition of Ireland, and the precedents in other departments, it is not wonderful that the mistake of imitating those examples was made, although, at that very moment, the political monopolies which they sought to rival in medicine were universally execrated by the Irish people, and virtually abandoned by cabinets of all shades and colours, from the cabinet of WELLINGTON to that of MELBOURNE! Men with far less sagacity ought to have seen that the system of exclusion which was not maintainable in the church, in the corporations, or at the bar, could not be sustained in the medical profession. It was not prudence but sheer stupidity to stand between the ruin and its fall; but from the quarter to which the party have all along addressed themselves, they seemed to think that if they could meet the statements and arguments of THE LANCET, their case was safe. Yet even had this journal latterly been silent, the impetus it had already given to reform could not have been retarded much longer. Public opinion had become too strong an enemy for the junto to cope with. Were the profession in Ireland likely to be satisfied with less than was to be conceded to the profession in England and Scotland? Did they imagine, for instance, that the fact that not a single individual belonging to the faith professed by the bulk of the Irish nation, fills (as we believe) the office of surgeon or physician to any Irish County Infirmary, would not be considered as a gross anomaly and a scandalous grievance? Was it to be supposed that the people of Ireland would be content with an University, the by-laws of which declare that no Catholic shall ever hold in it the office of Professor? As well, indeed, might Dr. GRAVES and his party expect that the sturdy followers of Calvin in Scotland would allow the episcopalians in that country to hold exclusive possession of all their universities and schools! And yet such is the proposition of the medical monopolists of Dublin!

But sooner or later will the inhabitants of Ireland either force open the existing institutions, or possess institutions of their own. They are intent upon this course. Their enemies force them to adopt it. We are perfectly aware of the kind of use that will be temporarily made of this announcement; nevertheless, the event cannot be retarded, however great may be the abuse cast upon THE LANCET by the monopolists. The question is not now, "Shall there be reform?" but "How far shall it be carried?" The spokesman, Dr. GRAVES, suspects this, or he would not have deviated from the old usage of treating public opinion with affected indifference, and admitted the necessity of reform, in his late "introductory" lecture at the *Meath Hospital*. The *extent* of reform will be discussed in the next harangue at that place.

IN our present Number will be found the first part of an extremely interesting paper by Mr. EDMONDS, of Regent-square, on the influence which age n the population, and locality as regards divisions of the country, exercise over the mortality of various portions of the English population. The new and valuable materials—of unexampled variety and extent—on which Mr. EDMONDS has exercised his abilities and his great industry,—are but little, if at all, known to the public; and the devotion of so much time and attention to them as his papers evince, renders the community, for whose advantage they are designed, greatly his debtors. The chief of those materials consists of a Parliamentary return of the ages of nearly 4 millions (3,938,496) of deceased persons, and of more than 10 millions (10,530,671) of persons living, also distributed according to the terms of their age. These numbers have been divided into 100 portions, the sexes of the individuals enumerated being distinguished in each county in which the individuals are stated to reside; and also in each of six large towns. The "law of their mortality," or a careful discrimination of the number of these individuals who have died at each distinct age, out of a given

number of persons who each lived to the same age, is the information which has been laboriously sought by Mr. EDMONDS, and ingeniously obtained by him from these materials.

In giving a place in our columns to the communication of Mr. EDMONDS, we are influenced by the impression, long ago entertained, that the subject is one of peculiar importance to the members of the medical profession. It can hardly fail to excite, indeed, a high degree of interest, local and general, amongst the people of this country; but to the medical community, especially, it affords an abundance of new and rich matter for reflection. The results themselves appear to us to be of high intrinsic value; but the exhibition of all the steps of the simple process by which they have been derived, may, perhaps, be considered of great permanent use as indications to the inexperienced in these matters, of the course which should be pursued in the investigation of the same, or of other similar materials. In medical statistics attention has hither to been confined chiefly to the relative mortality resulting from different diseases, and to the number of deaths and recoveries which have occurred among a given number of persons attacked by disease. The duration of the attacks, and the ages of the patients, are indeed now deservedly becoming objects of great importance. But observations of this latter nature can supply no measure of the health of a population, so long as we continue ignorant of the number and ages of the persons living from whom the diseased are taken. In order to obtain an accurate measure of health, it is essential that the number and the ages both of the living and the dying should be observed, and that the number who are sick at each age should be distinguished.

It may serve, perhaps, to increase the attention of our professional brethren to the documents which we publish, when they are informed that very great secrecy is preserved on the facts which they develop, by the remainder of the very few persons besides Mr. EDMONDS, who possess any real knowledge on the subject.

WE cannot find space for the insertion of a report of the proceedings which took place on Wednesday last at the *University of London*. The ministerial plan for founding a general university in the metropolis, is distinguished in many respects for its liberality; but the scheme will never receive our unqualified approval, unless the charter shall provide—after the institution is set in motion—for electing the future examiners by *concours*; nor, unless every candidate for the honours of the University shall be *admitted to examination*, simply on his furnishing proof of his having ever sustained a character of unimpeachable moral rectitude. The scheme *in its present form* will only add new strength and vigour to the odious, the infamous, certificate system. The letter, signed "A STUDENT," in the present Number of THE LANCET (page 388) relating to this subject, merits general attention.

THE facts relating to the late disinterment of a body at Moulton, and the consequences resulting from the discovery of the proceeding, are not unknown to us. We only hope, for the credit of the profession, that any one of its members who may be at present involved in the charge connected with the disgraceful transaction, possesses the means of freeing himself with honour from the implication. The affair is undergoing legal investigation, and we shall therefore abstain from saying anything farther on the subject at present.

THE LATE DR. HAMILTON.

(*From an Edinburgh Correspondent.*)

DR. HAMILTON, the author of the valuable work on the treatment of diseases by purgatives, died on the 27th of October last, having arrived at the eighty-eighth year of his age, and enjoyed, during that long period of life, an uninterrupted course of good health.

There were many features in the life and character of this able physician which are worthy of notice. It may justly be said of him, that he constructed one of the best works on practical medicine which appeared during his day; and he was, at the same time, one of the best practitioners of the Scottish metropolis.

JAMES HAMILTON was the son of the Professor of Natural Philosophy of the University of Edinburgh. Having had an excellent preliminary education, he commenced

the study of medicine,—the school of Edinburgh having at that time acquired its highest reputation, its list of professors being adorned with the names of MONRO, CULLEN, BLACK, and GREGORY.

Finding an unconquerable difficulty in even witnessing operations of surgery, Dr. HAMILTON commenced practice as a physician; and although he always had the reputation of possessing good sense and a sound judgment, he was very little employed in private practice; and the greater portion of his time was devoted to the duties of the Royal Infirmary of the city. It was not until he had passed the fiftieth year of his age that he enjoyed any professional reputation. He had never devoted himself zealously to any pursuit connected with the *science* of his profession; and he was even defective in its rudiments, having, like his contemporary, the second GREGORY, no knowledge of practical anatomy; and possessing, consequently, no accurate knowledge of pathology.

At about this period of his life he came forward with his work on "Purgatives;" and never was any literary production more cordially received by the profession. The views which he inculcated therein were at once adopted by a great proportion of British practitioners. It passed through one edition after another, at once establishing the fame of the author as that of a first-rate practical physician.

At the same time that HAMILTON was directing the attention of the medical world to the administration of remedial measures in the treatment of fevers, and ascribing many diseases of the nervous system to the alimentary canal, ABERNETHY was employed in pointing out a variety of local ailments, all of which were symptomatic of a derangement of some portion of the digestive apparatus, and to these also ABERNETHY directed all his therapeutic means. Each observer was employed in exploring the same country, but they travelled in different roads. Both observers agreed on all practical points; they both agreed in the fact that almost every disease is modified by, if not actually originating in, a deranged state of the digestive canal; and whether it be the stomach itself, the liver, or the large intestines, it is those organs to which we must direct our chief remedies in all the diseases of the human body. In confirmation of this important truth, we have only to look at the habits of mankind in civilized life. The irregularity of their meals, and the multitudinous variety of their food, contrived purely to gratify sensual desires, must constantly disturb the functions of the stomach, while the moral excitement to which mankind is exposed, must, in like manner, constantly derange the operations of the liver. It was to this portion of the alimentary canal that ABERNETHY directed particular attention in all his pathological investigations, whilst HAMILTON appears to have looked chiefly to the large intestines. To the congestion and accumulation of fecal matter in the colon, he attributed the chief affections of the nervous system, particularly chorea, but he considered that a vast number of symptoms of other diseases, originates in the condition of the alimentary canal. Both practitioners established their views by a reference to an immense body of facts, and their joint labours now form the groundwork of the practice of the ablest medical men in this country.

Much has been told, and truly told, of the absurdities into which these champions of the stomach occasionally fell, from the extravagant bias which their minds took on the subject, its diseases and remedies. To such an extent did Dr. HAMILTON carry some of his views, and so eager was he to inculcate the importance of examining the evacuated matters on all occasions, that in visiting his patients in the Royal Infirmary, he was in the habit of looking at the cloaca in every case. An old and waggish nurse who was employed in the Institution at one period, had no difficulty in providing an "evacuation" for the doctor's critical inspection, numerous as were the demands on her faculties of distinctiveness and arrangement. It was at once indeed a saving of trouble and a source of amusement to her to confuse the stools of the different patients, and then to present them to his notice indiscriminately. These deceptions, however, converted laughable errors into a useful conviction, on the minds of the numerous students who attended the Doctor's visits, that little was to be learned from the mere external characters of the evacuations from the alimentary canal.

As a private individual, Dr. HAMILTON was generally considered as a very primitive character. He was a perfectly upright and just man, but his generosity of disposition was not very conspicuous towards those who chanced to present claims on his benevolence. Though unmarried, he had the reputation of possessing a progeny. He was rather affected in his dress, continuing to wear a cocked-hat forty years after that form of beaver had been given up by every body except a few clergymen. He would not wear gloves even in the coldest days of which our northern climate can boast, and he always slept with the window of his room open. In personal appearance he was a little pleasant-looking body, and he generally contrived by his manner to make friends. His house being adjacent to that of Dr. HAMILTON junior, our well-known accoucheur, many amusing anecdotes are told of ladies making mistakes in the choice of their doctor, and it was often proposed that besides the distinctions of "senior" and "junior" on the doors, some symbol should be affixed to

the houses, characteristic of the favourite calling of each, in order that the victims of "Cupid" might not undergo an unmerited *purgatory*, by accidentally calling at the wrong house.

INTERCEPTED LETTER.

"Dear Sir Henry,—I always feel it to be an honour to receive a letter from you, and am ever proud to fulfil your commands. Your letter, most parts of which I read at a College meeting, was most gratifying. I am every day more convinced that you are not even yourself as well aware of your public as you are of your private professional value. We all look up to you, justly, as the chief pillar of our venerable institution; and we know that when *you* fall so must Pall Mall East. I rejoice in the amendment of your health and spirits, which at the last College meeting you attended we all observed had faded a good deal. Wiston Hall is a delightful spot, and I hope you will remain there until the close of November, in order that both your mind and body may be recruited. My solicitude to see you once more amongst us, prompts me to take the liberty of cautioning you not to make too free with your constitution, and expose yourself too much to the endearments of the fascinating circle in which you move, and have been so fondly caressed. I cannot help recalling to your memory the sage advice which you gave some years ago to the gal*lant* Duke of Wellington, and as *you* are much more advanced in years, the same precept may now be to you even more applicable. I am confident that by tender nursing and great command over your passions, your mind and body may yet last a few years. The aged are not less apt to become the victims of female society than youth, and if you at your time of life were to take to yourself a young wife, we should soon lose your valuable services. It is my high consideration of those services that prompts me to write to you on this delicate subject.

"I don't know that I have much news to tell you. All of us will join you heartily in keeping things at the College *in statu quo*, and we are every one confident that your ingenious and comprehensive mind will contrive a tub for the whole, and that you will easily manage to amuse these pettyfogging licentiates with another reform phantom. You have no one to fear, as all of us will be glad to keep you in the president's chair. There is in fact no other man in the profession high enough in rank and science to fill it with the same dignity and credit. The conspirators I think are paralysed. Maton, poor creature, certainly aspired, but he is gone to a better world. Had he been a more formidable rival, we should not have suppressed the facts respecting the indecorous part of his life. A married Fellow, with legitimate offspring, might have been something; but the least public suspicion of immoral habits, unfits any one to be an occupant of the President's chair. Such a person could never have overthrown a man with your high moral feelings. Warren, whom you reasonably considered as an opponent, will soon leave this wicked world, for he is very very ill; so instead of fearing him, you may pray for him.

"I have done all I could to obtain information about the schools. The different lecture-rooms have been open some time, and I wish my statements respecting several of them could be more gratifying. But I must not deceive you. The medical pupils of the London University have increased prodigiously. Last year there were 300, but this year the number is not less than 500. The other schools have suffered a proportionate diminution, some of them having been hit most severely. St. Bartholomew's, notwithstanding all Stanley's efforts, are 83 *minus*.

At the Borough hospitals they are making a convulsive struggle, but all the palm oil which King Harrison can supply, does not seem to make the rusty wheels of Guy's go quicker. At St. Thomas's, however, the classes are a shade better. The reason is, I understand, that they are more liberal in their conduct to strangers. At Guy's their demeanour to visitors is quite ungentlemanly, and contrary to every rule observed at the other and more liberal recognised hospitals. They are machinating a quarterly journal of Reports. Such a thing is already advertised at St. Thomas's. But they are a dozen years too late. Had they been wide awake in 1823, they might have prevented the inconvenience of criticism with reporting. But they say that the unfortunate *Gazette* is read by so few that it does not answer the purpose of the hospital surgeons. These *pure* journals, however, must fail, as of course they will only contain successful cases, and will give none of the bungling practice. Nobody but a blockhead would publish an unsuccessful case.

"The Westminster School is as low as low can be. I conversed with a gentleman who went to hear one of the new-fashioned *clinicals*, and who, after waiting for three quarters of an hour, took his departure, because no other soul but the lecturer attended. Guthrie is sliding down rather fast, though he was never far up in the estimation of the competent judges. You will find a melancholy scene described in The Lancet. All true! The College have kept the Charing Cross concern out of competition with their own hospitals, by refusing to recognise the practice. Our St. George's friends have met with the fate which you foresaw. The 7000*l.* sunk in brick and mortar, will never again be converted into currency. The two

contending parties have about shared equally in the pupils, each having from twenty-three to twenty-five. BRODIE, poor fellow, would not hearken to good advice and give up the opposition. LANE, who is as sharp as a needle, has certainly got the best shop, having spent upwards of 1500*l.* in putting the original school in order. The Middlesex must be considered as a failure. This I regret only on your nephew's account, for Sir Charles is a bitter enemy of things as they are, though, loud as he talks out of the College, we never hear of any of his efforts in doors to help the reformers. The University hospital has kidnapped every pupil in that quarter. By-the-by a new medicine, called *creosote*, is all the rage here. Have you ever heard of it?

"I saw TUPPER last night. He appeared low, and said the trade was *dull*, that he was *looking out* for patients, and that the old families who used to permit all their domestics to indulge in the *draught* system at the master's expense, now oblige them to go to dispensaries, or have their own doctors, which makes a serious difference in the Christmas bills of all the top-apothecaries. He says that no one now ever wants a pure physician, and that none of his patients will stand a *regular attendance* any longer. We are all fully sensible of the complete change which has taken place in the guinea trade, and every day see the necessity more and more of becoming, in reality, general practitioners.

"I have sent you per coach WARDROP'S book on Blood-letting, which was published yesterday. I think it will please you. It is full of practical information, and contains what are to me many views which are quite original; but I should like to hear your opinion of the work. Favour me also by saying what number of copies I should order for our library, as it will be much called for, for it is a work not for a cursory perusal, but for attentive study and calm deliberation. There is one point in which you may obtain some useful information from it. I allude to the *quantity* of blood to be taken. I remember a lady, who was present at one of your visits, telling me of the happy effects produced by your nice discrimination of the requisite quantity to be removed in the case of the old Dowager Duchess of R*****D. She said that after repeated most accurate examinations of the pulse, and of the alvine and urinary discharges, *a la Pront*, you prescribed the loss of *four* ounces of blood by cupping, and then departed; but on returning to your noble patient in two hours, and having first ascertained that the cupper had not arrived, you then, with your customary tact, expressed an unusual interest in the case, saying 'that your anxiety was more intense than it had ever been for any patient before,' and that 'after having contemplated, reviewed, and turned over again and again in your mind every particular of her condition, you had come to the important determination that the quantity of blood to be removed should be only *three ounces and a half*.' I shall never forget the favourable impression which your conduct on that occasion made on all concerned; and for years after, the noble family of R*****D could never hear your name mentioned without a tear of gratitude rolling down their cheeks.

"I am glad you are riding a quiet pony. What a frightful escape little BRODIE had in the Isle of Wight the other day! I trust you will have the kindness to let me know about the time that you will come to town, and say if I can do you any little kindness at Kensington. Do you think there will be a chance of getting up some meetings at the College this season? There are many difficulties which you alone can surmount, if surmountable. I remain, dear Sir Henry, ever your very humble and obliged servant,

"W. Mc. M.

"Half-moon Street, Oct. 1835."

LONDON MEDICAL SOCIETY.

Monday, Nov. 30, 1835.

Dr. WHITING, President.

TREATMENT AND EFFECTS OF FRACTURES.

MR. BRYANT placed on the table a specimen of fracture of the femur, of which he gave the following history: A female, aged sixty-seven years, much addicted to drinking, occasioning considerable feebleness of constitution, was knocked down by a boy, when the left trochanter forcibly struck against a curb-stone. She was unable to rise, or bear the least weight on the limb. She was conveyed home and put to bed, and simply kept quiet. On the 27th of October, three days after the accident, she was brought to the infirmary, when the injured limb was found to be two inches shorter than the other. The foot was everted, and on rotating the limb, distinct crepitus could be heard and felt at the upper part of the thigh-bone. The woman was placed on a fracture bed, and the limb on a double-inclined plane, with splints and bandages to maintain the broken parts in apposition; and thus placed she appeared easy. On the following day she became extremely restless, pulled off the bandages and apparatus, and obstinately refused to have them re-adjusted. He (Mr. B.) then placed the limb on its outer side, with the knee bent on a pillow, insisting on perfect rest being kept. She remained easy and comfortable, but became every day more feeble, although a supply of stimulus, to which she had been a long time accustomed, was allowed her. The skin was hot, the

pulse rapid and feeble, and the tongue dry and brown. The symptoms continued increasing up to Saturday, 28th November, when she died. On examination, a fracture was found through the trochanter; the neck of the femur was driven into the cancellated structure, and the trochanter minor was broken off. The fracture was external to the capsular ligament, but no attempt at union had occurred. Cases, however, of fracture of this description were described by Sir Astley Cooper, where union did follow. His (Mr. Bryant's) object in bringing this case before the Society was to engage its attention on the treatment of such injuries, which he thought was a very desirable object, especially at the present time, as an excellent paper had been lately published in THE LANCET, by a Mr. Radley, who had advanced, very ably and strongly, the treatment of fractures without splints. The plan recommended by Mr. Radley appeared to have been so successful with Mr. R., that it could not be otherwise than worthy the attention of every medical society. Two other specimens of fracture, on the table, were taken, the one from an old man aged sixty years, and the other from a woman aged sixty-three, the injuries arising, no doubt, from the extreme fragility of the bones at their advanced ages; and he (Mr. B.) was very sceptical whether the practice of confining the patients on the back, with the limb extended, was a good one. In fact he was led to consider that great advantages would result from the surgical treatment recommended by Mr. Radley. He (Mr. Bryant) did not consider that any benefit would have ensued in the present case if mechanical contrivances had been adopted; and in such cases he regarded the position on the side, with the knee bent, resting on a pillow, the most likely to answer the object of the surgeon. The Society's time, he believed, would not be ill-spent in giving this important subject an attentive consideration. In the case of the female first mentioned to-night, he believed that the accident had but little to do with her death; for but very slight inconvenience arose from the fracture. He, perhaps, might account for the fatal event correctly, by asserting it to be the woman's having become habituated to the use of ardent spirits, which having been in the first instance withdrawn, low fever ensued, which, in its turn, by degrees, occasioned the feebleness under which she sank.

Mr. JONES was persuaded that patients having received severe injuries, died, in innumerable instances, from a peculiar kind of slow fever that succeeded, without disclosing any structural change. Irritation, also, he regarded as a frequent cause of death, arising from fracture, for instance.

Dr. JOHNSON was disposed to think, that the death of Mr. Bryant's patient was not referable to the injury, but to the shock which the whole nervous system had sustained. He had seen many persons die from falls which had produced no fractures, in the course of a very few days.

Mr. DENDY pronounced Mr. Radley's plan to be no plan at all, for it permitted the patients to do as they pleased, when, if there were any disposition to displacement of the disunited parts, the treatment without splints would not succeed. In Mr. Bryant's case little could be done, but he (Mr. D.) would have added a pelvic strap, to keep the pelvis fixed.

Mr. CLIFTON considered the principal source of irritation in very old people, to depend on the confinement and restraint to which they were subjected after the receipt of an injury.

Mr. PILCHER attributed the development of the irritation, rather to the confinement than to the abstraction of the stimulants, the former leading to an interruption of the functions of the body, and finally to a disorganization of the viscera.

Dr. JOHNSON maintained that the irritation or mischief resulted from the shock which the constitution had received.

An animated discourse ensued, terminating, however, in each member regarding his own theory as most feasible, in accounting for the phenomena occasioning death in such cases, and the Society soon afterwards adjourned.

OUR REPORTER remarks on the note of Mr. Hooper in the last LANCET, "that he did not anywhere state in his report, that Mr. H. had said that, 'the exostosis on the tibia of the patient well illustrated the proposition laid down by Mr. Abernethy;' but, rather, what Mr. H. himself states in his note (page 349), viz., that cancer will invade every structure of the body. Although the word exostosis was perfectly separate from the sentence, still it had reference to it; for, as stated in the report, not only soft parts were destroyed by cancer, but bony parts also."

WESTMINSTER MEDICAL SOCIETY.

Saturday, Nov. 28, 1835.

Mr. RICHARD QUAIN in the Chair.

DEATH FROM TAKING MORISON'S PILLS.

DR. JOHNSON, in answer to a call from Dr. Granville, detailed the post-mortem appearances of Mrs. Sarah Porter, aged 39 years, who died from effects caused by taking Morison's pills. The particulars of the case were given in our report from the London Medical Society, Nov. 33, the day on which she died. The abdomen was rather tumid and distended; peritoneum healthy;

stomach inflamed, presenting slight abrasions of the mucous coat; no changes in the duodenum; jejunum and ileum twisted in different parts one with the other, and the morbid parts had assumed the colour of dark mahogany, with distinct demarcations of healthy surfaces; the vessels were injected, and from their capillary extremities blood was extravasated; there were no traces of ulceration, the mucous surface being simply abraded. The parietes of the heart were rather thicker than usual, probably the commencement of an hypertrophied condition. The bloodvessels of the brain were congested throughout, but there were no other morbid changes affecting the substance of that organ. In answer to a question whether the pills had ceased to produce any action some time prior to death, Dr. Johnson said that the medical gentleman in attendance, and the friends of the deceased, had informed him, that ten days prior to her death, she was recommended to give to one of her children, who was troubled with palpitation of the heart, some of "Morison's Pills;" she complied, but the child got worse. Presently the mother had pain in the head, and she also took them, in doses such as we have before described, producing, shortly after, distressing vomiting and violent purging; and her adviser remarked, that the more actively they operated, the greater number of pills it was necessary to take! An increase in the violence of the symptoms necessarily followed. The friends finding her becoming rapidly worse, sent for Mr. B—, a medical practitioner, who found her comatose, in which state she remained up to the period of her death, four days after commencing to take the pills. The last day or two prior to the fatal event, the bowels had ceased to act, and at the post-mortem examination they were simply distended with air. He (Dr. J.) came to the conclusion that death was occasioned by the pills, from the fact that some portions of the tube were highly diseased, whilst other portions were perfectly healthy; and where the lodgement of the irritating substances had taken place, there the changes were evident, and the contractions appeared as if tied with a piece of silk, while there no morbid adhesions had supervened; these contractions, when found in the healthy portions of the canal, were a positive evidence of their having been produced by the direct application of an irritating substance. But he did not wish it to be understood that he believed that other powerful pills, taken in the same enormous quantities, would not have occasioned the same result. (The virtues of this quack medicine, however, are said to reside in large doses.) The affection of the head was attributed to the large doses of Ext. Conii,* which the pills, by analysis, were found to contain.

Mr. Verral said he had been requested to attend an old patient, who, he was convinced, had also met with his death from the same cause; and in whom the pills and other aperients, towards the close, ceased to produce any action. After death an immense mass of the pills and of oil was found wedged in the intestinal canal, in so putrid a state, and emitting so powerful an effluvium, that the like he never before encountered.

MEDICAL MAGNETISM.

Dr. Johnson, in a brief address, re-introduced this subject, but it occupied attention only for a short time, the conversation consisting of a few questions that were put to Dr. Schmidt. The discussion of the previous night, Dr. Johnson said, proved to the Society that medical men were not the only class of philosophers who differed. As much variance of opinion prevailed to-night as on the last evening of discussion.

Dr. Ritchie denied that the magnet, by itself, could produce any therapeutic effects.

Dr. Schmidt maintained that it could, if the poles were placed according to his arrangement.

Dr. Ritchie thought it quite a joke that Dr. Schmidt should know how to manufacture magnets more powerfully, and place them more remedially, than other persons.

Dr. Epps replied, that a gentleman had offered to Messrs. Watkins and Hill, of Charing Cross, 100 guineas if they would produce magnets having the like power.* They tried, but at last were compelled to acknowledge their incompetency; and he would now propose that Dr. Ritchie and Dr. Schmidt should each take a certain number of pieces of iron, and try who could produce the most powerful magnets.

Dr. Schmidt offered any medical gentleman who doubted his competency to perform what he had described, or who believed that the magnet could not produce physiological effects on the human body, to bring to his (Dr. S.'s) residence, any poor and sick patients on whom magnetism might be tried, the visitor judging for himself of the results, for he had no secrets to conceal. He had abstained from quoting cases cured with the magnet within the last six years. He would, however, briefly state that he knew a lady, aged 22 years, afflicted with amaurosis for several years, and who, during the last eighteen months, was completely blind,

* We believe the manufacturer, in order to baffle the analyst, changes his formula (with the exception of the ingredients that give to his pills their powerful drastic aperient quality), as it best pleases his fancy, when preparing the means of deception, for no two analyses furnish the like product.—*Rep.*

* We take this opportunity of asking if Dr. Schmidt withholds any information relative to his mode of constructing magnets?—Ed. L.

who was seen by several eminent physicians, without benefit, but in whom the magnet produced a speedy and perfect cure. It would avail nothing to detail cases within these walls, unless authenticated by the presence of the patients themselves. Many remedies there certainly were in the pharmacopœia, but the profession, nevertheless, were not overstocked with remedies.

Mr. HALE THOMPSON said he was authorized to offer to Dr. Schmidt the use of the Ophthalmic Institution, where he might select as many cases of amaurosis as he thought advisable, and in order that the magnet might have a fair trial, the sole management of the selected cases should be entrusted to his care, and he (Mr. T.) would give to the Society an impartial statement of the results.

The offer was accepted, but not so willingly, apparently, as to prevent Mr. KING from believing that a reluctance existed on the part of Dr. Schmidt so to test the efficacy of the magnet. Mr. King considered that more effect would be derived from electricity, though whether the magnetic fluid differed from the electric he was not aware.

Dr. SCHMIDT said he had found injurious results to arise from the application of electricity in the treatment of diseases, and, moreover, he had not so great a control over the electric as the magnetic influence.

Dr. JOHNSON advised Dr. Schmidt to accept the offer to make experiments at the public institutions, but to be careful in the selection of patients, especially those affected with amaurosis, for it was impossible, he considered, that any remedy could accomplish a cure when the cause arose from organic changes.—On the whole, the Society seemed to form a more favourable opinion of magnetism this evening, the interest in the subject being heightened when it was announced that Dr. Faraday had purchased some of Dr. Schmidt's magnets (which Dr. S. considered as a proof that his magnets were very superior) and examined his experiments, remarking that his views were new, on some points, and no doubt would lead to important results, he (Dr. F.) stating that he should more fully investigate the subject. The adjournment of the Society then took place.

MEDICAL CONTRACTS IN THE NEW UNIONS.

LETTER FROM MR. CEELY OF AYLESBURY,

To the Editor of THE LANCET.

SIR,—I have never been among the number of those who have derived the least satisfaction, or indulged any hope, from the declaration of the Poor-Law Commissioners to the Kent deputation:—"That the guardians of the poor of the respective parishes should not be prevented entering into as many contracts with medical men as they pleased,* because, in the first place, in the rules and regulations originally issued for boards of guardians by the commissioners, a clause exists, relating to medical relief, permissive of such authority,—a clause which, in an official communication on the subject, dated June 20th last (now before me), their secretary states, "he is instructed to declare, empowers guardians to exercise an *entire discretion* with regard to the medical appointments." In the second place, experience has taught me to entertain no very exalted opinion of the discretion of such bodies in general on such a subject. In the third place, I am unwilling to respect the declarations, and unable to appreciate the authority, of a central triumvirate which, though incessantly disclaiming all interference in the medical arrangements, and for ever alluding to the grant of these discretionary powers, at the same time permits most of its itinerant assistants to be actively and relentlessly engaged in prejudicing, insulting, and degrading the medical profession, often *prompting* guardians to the execution, and *always* supporting them in the infliction, of the most flagrant injustice on numbers of its worthiest members.

For these reasons, also, I could not participate in the surprise expressed by you, in a recent Number of THE LANCET, at the proceedings relative to medical contracts, still manifested by the guardians of several unions, much less can I concur in the correctness of your strictures of the 21st inst. on this subject.

I could relate many instances in different counties, previous to the Kent deputation, where guardians, uninfluenced by assistant commissioners, have exercised this discretionary power with humanity to the poor and justice to the medical profession. I could also mention a multitude of instances, both before and since that event, where, in spite of the above recited discretionary clause, the secretary's authorized explanation, and the reiterated official assurances in the House of Commons, assistant commissioners have most illiberally and unjustly interfered in these matters, either dissuading them from the pursuit of an enlightened and judicious course, pandering to their parsimony, or ministering to their meanness. Boards of guardians, in general, need not such *assistants*; and it is equally true, that had those bodies been left to their own feelings, knowledge, and discernment, infinitely less evil, and much less injustice, would have been perpetrated in the medical arrangements.

* Vide LANCET, No. 627, p. 747.

In the Wycombe Union, formed some months since, the guardians refused to admit of medical districts, though urged to it by Mr. Sub-Commissioner Gilbert. He, however, succeeded in persuading the Board, after having unanimously agreed to the adoption of a low scale of graduated payment, proposed by a medical man of acknowledged talent, experience, and liberality, to abandon their design, and force the medical men, by modern threats, to accept a shamefully inadequate stipend.

This functionary, doubtless, in laudable rivalry with his associates, declared publicly and privately, "that the medical men of Buckinghamshire had given him so much trouble that, on all occasions, he would urge the guardians to procure medical officers from London"—"that although they had caused him most trouble, yet he could always subdue them by introducing very clever young men from Somerset House, where there was always a long list of them in the possession of the commissioners."

The Aylesbury Union comprises forty parishes, and contains a population of more than 22,000, with districts most preposterously and perniciously arranged in reference to the medical duties, and is it not less than twenty miles in extent in one direction. During the formation of this union, that sagacious gentleman Mr. Gilbert gravely inquired "whether one medical officer would not suffice for the entire Union."

When the Union was formed, in September last, he took an active part in dividing it into four arbitrary districts for the relieving and medical officers, and suggested terms for the latter to which none of the resident practitioners could accede. The Union was disposed of to three individuals; two from London, one having two districts, who are engaged to discharge the medico-parochial duties lately performed by *sixteen!* Whether the medical men in Devonshire have proved more compliant than those of Buckinghamshire, or whether this gentleman *there* continues his pragmatical proceedings, our brethren in the South, I dare say, can inform you.

In Hertfordshire, after a contract had been ratified, the assistant commissioner, Mr. Adey, refused to sanction it till 7*s.* 6*d.* was substituted for 10*s.* 6*d.* for midwifery, in and out of town. In Bedfordshire, guardians have been officially informed that the *commissioners* would not allow more than 7*s.* 6*d.* for this important item. This gentleman has publicly declared, "that, notwithstanding medical men now complain of inadequate parochial remuneration, they will receive much less next year."

In the Thame Union, where the medical appointments have been made within this last month, and where the *highest* tenders were accepted, the Assistant Commissioner, Mr. Gulson, and the Central Board, interfered. The former offered a London medical man at a lower rate, at the same time declaring that the election just made would not be confirmed. The latter proposed a sum scarcely equal to two-thirds of that claimed by the successful candidates.

The guardians, however, after much discussion, would not accede to this, but ultimately were induced to propose a considerable reduction of the original terms. Contracts, on these abated terms, were then ratified by the guardians, for *twelve months*. Within these few days, however, the Central Board—the Poor-Law Commissioners—have signified to them in writing, "that, under the circumstances, they will allow the medical appointments to continue only *until the end of the parochial year;* at the same time they consider the remuneration proposed too high, and such as they will not feel justified in sanctioning in future."

If further evidence could be required to show the spirit actuating the Central Board towards the medical profession, and to declare the hopes we may entertain from that quarter, surely their last Report to Lord John Russell* will furnish abundance. That report can be proved to be no less replete with unwarrantable aspersions and unfounded imputations, than distinguished for the arrogant folly and the ignorant credulity of its subordinate concoctors. It may, in its characteristics, stand unrivalled; but I am happy to say it will not remain unanswered. I do hope, as you suggest, that it will at once engage the serious attention of the profession in general; and that, since his Lordship has the bane, he may also have the antidote.

By extensive union, cordial co-operation, and an appeal to the Legislature, supported by authentic facts, existing in abundance, I cannot doubt that the honour and respectability of our profession will be best vindicated and most successfully maintained.

I am, Sir, your obedient servant,

ROBERT CEELY.

Aylesbury, Nov. 30, 1835.

METROPOLITAN DEGREES IN MEDICINE.

To the Editor of THE LANCET.

SIR,—I perceive by the daily papers, that it is the intention of the Government to "incorporate by charter certain persons eminent in literature and science, as the 'University of London,' with power, after examination, to confer degrees in arts, law, and medicine." It is also stated, that candidates for degrees must produce testimonials of having *gone through a regular course of study*, either at the "King's College," or

* Vide LANCET, No. 631, page 49.

at the institution now denominated "The London University."

Now, Sir, this does appear to me to be neither more nor less than a continuance of the old system, and to imply that the examination for degrees, at the new "University of London," will be as useless as that process has hitherto been in other quarters. If it be not a perpetration of the old scheme under a new name, why is it necessary that a regular course of study should be "gone through" at any institution whatever? The circumstance that a candidate has passed a certain number of years in professedly educating himself anywhere, is no proof of the ability or attainments of the professedly educated individual; and it is a fact which cannot be disputed, that many persons have attained degrees, in our two great universities, without presenting any solid claim to the possession of those marks of distinction. So that it would seem already, that neither certificates of having studied, nor the fact of having passed an examination under the influence of the old system, is any guarantee of qualification to hold a diploma.

But suppose we admit that the examination will prove so good, that none but competent men can pass the proposed examination. What then is the use of certificates of "courses of study?" Why should only those who can afford to obtain their knowledge at a university, be qualified to become *candidates?* The adoption of such a system necessarily excludes all but the comparatively rich. The want of means must prevent many able young men from pursuing their studies in an English university, but who, in the retirement of a private study, would attain a degree of knowledge which is far beyond that possessed by the majority of those who go through "courses of study," and obtain degrees. It does not matter *how* knowledge is obtained,—whether in the solitude of Welsh mountains, within the walls of a university, or amidst the din of a metropolis,—whether in Wales, France, or England. If the knowledge be but obtained, its possessor is equally worthy.

But, perhaps a fear is entertained by certain parties, that such an arrangement would increase the labour of an examination, because much which ought to be proved at an examination, is *now* taken for granted, or would be so, under the "course-of-study" system. But, even were more labour required, such labour would not be lost, for the worthiness of the possessor of degrees would thus be *ensured*. We might then look upon a degree as a thing of value,—as the evidence of a fair and adequate test of attainments,—satisfied that whoever possessed one, would first have proved before men of talent and renown that he had deserved it.

You, Sir, have distinguished yourself in the ranks of rational reform, and have throughout exerted yourself to obtain a pure mode of government in our metropolitan collegiate institutions. I have, therefore, no doubt that you will insert these remarks in THE LANCET, and give the liberal side of the question all the support in your power. I am, Sir, yours truly,

A STUDENT.

London, Nov. 28, 1835.

MEDICAL REFORM IN IRELAND.

To the Editor of THE LANCET.

SIR,—In the last Number of your truly independent journal you have been pleased to notice and comment on a lecture lately delivered by me in the Peter-street School of Anatomy, Medicine, and Surgery, in which I introduced the important subject of medical reform. Judging from certain passages in the commentary I am induced to think that your reporter must have either mistaken my meaning, or otherwise (unintentionally no doubt) fallen into error in reference to some points connected with the discussion of this very interesting topic. In order to prevent misconception or misunderstanding in the minds of the readers of THE LANCET, may I beg you will give insertion to the following explanatory observations in your *next* Number? In the commentary the following passage is to be found:—

"So far, therefore, as the act of divesting the theme of its imaginary horrors, and of calling general attention, by personal appeal, to its examination, is a meritorious one, we cordially concur in the favourable estimate which we hear was formed by his auditors of the manner in which Mr. Ellis discharged the duty he had undertaken. But if our report of the lecture be correct, we cannot state that we think his enumeration of medical abuses was so comprehensive as it should have been in a discourse professedly devoted to their discussion. Some of these omissions are, indeed, so remarkable that we mean to notice them. In our experience every sound reformer has always looked upon the system of *compulsory* apprenticeships as one of the baneful usages of the surgical profession in Ireland. They have also ever thought the scale of fees to witness hospital practice most oppressively and iniquitously unjust. Yet, upon these two important items in the catalogue of medical abuse, no opinion was given in the oration of Mr. Ellis. In these omissions, which we presume were accidental, he was neither just to himself, nor to the question which he designed to advocate, and he ought to have foreseen that the absence of condemnation of such glaring and notorious features in the picture of medical economy in these countries, might, with seeming justice, expose him to the unworthy suspicion that he was performing the play of Hamlet 'with the part of Hamlet left out by special desire.'"

I consider the first sentence of this passage complimentary to a degree quite beyond my humble deserts; for, in my opinion little praise is due to a person for the mere discharge of what he conceives to be a duty which he owes to the public, the profession, and the situation which he may chance to occupy in society. If I were so fortunate as to obtain the approbation of my hearers

on the occasion alluded to, my success is much more attributable to the "good cause" in which I was embarked, than to the ability of its talentless, though zealous, advocate. In the second sentence you candidly assert that "if *your* report of the lecture be correct," that you do not think that my "enumeration of medical abuses was so extensive as it should have been, in a discourse professedly devoted to their discussion." Here, in the first instance, you have been led into a mistake; for, although the advertisement to which you make allusion in the first part of your commentary did announce that the "important subject of medical reform" would be *introduced*, it did not state that the lecture would be *exclusively* "devoted" to the discussion of medical abuses; and, indeed, even if it were, *your* reporter ought to know that "the catalogue" is much too long to be elaborately discussed within the short period usually allotted to a lecture. The fact is simply this; the lecture in question occupied *an hour and three-quarters* in its delivery; the first half-hour was spent in general observations on the subject of medical education, and the remainder of the time was devoted to the exposure of some of the most glaring of the countless and multifarious abuses with which the medical corporations, and the hospital and dispensary establishments of these countries, abound. You will, therefore, I trust, give me credit for veracity when I assure you, that if I have failed to satisfy your reporter in the length of the discussion and in the character of the numerous topics it involved, the omissions complained of, *if real*, could be more fairly attributed to want of time than any disposition on my part of suppressing truth "by special desire," as delicately insinuated by the commentator on "the report." He states that some of these omissions are indeed so remarkable that he means to notice them, and then goes on to say, that in his "experience every sound reformer has always looked upon the system of *compulsory** apprenticeship as one of the most baneful usages of the surgical profession in Ireland." Now, Sir, I am quite at a loss to understand what the writer of this passage means; there is no such thing, nor has there been since 1828, as "a system of *compulsory* apprenticeships in the surgical profession in Ireland." Am I, therefore, to be censured because I did not betake myself to the tomb of a defunct monopoly, and exhume therefrom, as a topic for discussion, the ashes of an abuse which has had no material existence for the last seven years, when I found myriads of living and substantial ones obtruding themselves on my attention, and loudly demanding exposure? Would time permit, or common sense tolerate, such a line of procedure?

On the subject of hospital fees I believe I did not make any direct or specific observations; however, by a reference to the report, "if it be correct," it may be inferred that I am favourable to their *total* abolition, *provided* the medical officers be *otherwise* remunerated for their attendance, and the time and trouble bestowed in giving clinical instruction to the pupils. When speaking of the French system of medical education I expressed an opinion favourable to it, and explained at the same time that there were no fees demanded from students for the privilege of witnessing hospital practice in Paris. I may here perhaps be permitted to add as an item in my defence against the insinuated charge of wilful omissions, that the pupil-money received at the Jervis-street hospital, to which I belong, is applied to the support of the institution, whilst the surgeons are left unrequited, not only for their professional attendance on the patients, but likewise for the time and trouble consumed in imparting clinical instruction to the pupils.

The last charge advanced is contained in the following passage. "In speaking for example, of hospital appointments derived by 'purchase' and 'descent,' our report of his lecture describes him to have said that the persons who are concerned in this infamous traffic and practice are warranted in their proceedings. Making every allowance for the intention of condemning the crime and saving the transgressor, which we consider to have been the object of Mr. Ellis in this nice discrimination, it is unquestionably true that no man has a right to enter into a compact which will be of detriment to the public welfare." Here again the reporter must have mistaken my meaning, if he supposed that I meant either to justify or defend in an *unqualified* manner, either the actors or the means by which the appointments alluded to are generally effected. However, when I express myself thus, I by no means admit the correctness of the assumed premises that all such appointments must necessarily "be productive of detriment to the public welfare." For instance, suppose a case in which an hospital surgeon, or physician, has, either from excess of occupation, indisposition, indifference, or any other cause, performed *no* part of the duties of his station, with the single exception of receiving annually *his* dividend of the pupil fund; yet he will not resign, and, unfortunately, he has enough of friends on the *managing* committee to save him from expulsion. Now, would it not be more conducive to the interests and the objects of the institution, and "the public welfare," that such a person could be *induced*, when he cannot be *compelled*, under the *present* system, to resign, and in this way make room for a com-

* This was an error of the press. The word accidentally obtained its place in the page, after the mark of erasure was affixed against it in the proof.—Ed. L.

petent successor who would feel both a pleasure and a pride in faithfully discharging the important offices which *his* sense of duty would impel him to fulfil? This is by no means an extravagantly conceived, or imaginary exemplification, either of contempt of obligations seriously imposed, or of human depravity, as exemplified in our medical locusts. I could, in support of my argument, name, if necessary, an individual who has *practically* illustrated by his conduct for the last twenty years the truth of the position I here lay down; and I could likewise mention three or four instances of *salutary exchanges* which have taken place in hospital appointments, although they were effected chiefly by *private* arrangement. Give me leave to add that, in my opinion, the man who obstinately perseveres in *monopolizing* an important medical situation, and will, in the dog-and-the-manger fashion, neither do the duty, nor resign, but takes advantage of his spurious interest and vitiated influence with a corrupt committee, is much *more* culpable than a person who would vacate even for a pecuniary consideration. As long as the present system is permitted to continue in operation, and human nature remains what it is, *moralize* as we may, *nepotism*, *patronage*, and *money*, will exert a preponderating influence in determining medical appointments, both in Great Britain and Ireland, and the "infamous traffic and practice" will go uninterruptedly on, until restrained by the powerful arm of legislative enactment. It appears to me that the adoption of the principle of the concours in reference to hospital appointments in these countries would be the most efficacious method of putting a stop to the "flagrant abuse" complained of; and that it is only by rendering the commission of "the crime" impracticable, that "the weapon" can be effectively wrested from "the aggressor." This is the opinion I expressed, and the doctrine which I endeavoured to promulgate, when delivering my unimportant sentiments on the important subject of MEDICAL REFORM.

I have the honour to remain, with great respect, your obedient humble servant,

ANDREW ELLIS.

47, William-street, Dublin,
Nov. 28th, 1835.

DELIRIUM WITH TREMOUR.

To the Editor.—SIR,—Your correspondent respecting *Delirium Tremens*, in your last number,—and who need not have appeared anonymously in the matter,—is certainly correct in supposing that I have inadvertently overlooked the reference I have made to Dr. Elliotson's lecture. I find that his lecture was published on the 12th of January 1833, and that my article on the subject was in the printer's hands at the time; this and other references having been afterwards added in the proof. Upon referring to my article, I observe, that the names I have suggested among the *synonymes* are, "*Idiopathic Delirium;—Delirium Tremefaciens;*" and I have accordingly placed the word "*author*" after them. As to the pathological doctrine and division I have claimed, I remain of the same opinion as before. Upon looking into my friend Dr. Elliotson's able lecture, I found that part of it which is devoted to delirium tremens methodically arranged into "*Symptoms*," "*Causes*," "*Diagnosis*," and "*Treatment*;" but I could not find the distinctions in question under either of the heads "*Symptoms*" and "*Diagnosis*," where I expected to find them; and it was not until I had arrived nearly at the conclusion of the "*Treatment*;" that I found the subject adverted to, and there only in an incidental manner. I may take occasion to state, that the first case which directed my attention to the distinction in question occurred in a dispensary patient in 1821; and that, about ten years ago, I attended, within a few months of each other, three cases of the disease, with Mr. Houlton, of Lisson Grove, a gentleman well known to Dr. Elliotson and myself for his extensive literary and scientific acquirements, as well as for his experience; and one of those cases presented the inflammatory characters, and occurred under the circumstances which I have detailed at length. I do not dispute that the distinction was made about the same time by Dr. Elliotson and myself; but that I have been indebted to his lecture I unequivocally deny. Whoever will first read Dr. E.'s lecture, and immediately afterwards my articles on "Delirium," and "Delirium with tremour," will find internal evidence of my originality. If I may judge from my feelings and habits, I should infer that, in glancing—for I can do no more in many instances, even where I make references—over this lecture, I perceived nothing which particularly struck me, otherwise I would have taken further notice of it; and that the topic in question should have escaped me is not surprising, when the head under which it was noticed, both briefly and incidentally, is taken into account.

I am, Sir, your obedient servant,

JAMES COPLAND.

Bulstrode-street, Nov. 30, 1835.

CORRESPONDENTS.

WE have received another communication from Mr. ROGERS, in reply to the letter of Mr. G. Bury, relative to the proceedings at the late inquest at Farnham. We think we shall best consult the ends of justice by excluding from our columns any further remarks on the subjects in dispute until we have obtained on official copy of the depositions taken at the inquest. It is not necessary to say one word in justification of the conduct of Mr. ROGERS. He has manfully stepped forward to protect the character of an innocent man; and in doing so, will obtain the approbation of every just intelligent member of society.

WE have not space for the letter signed *C.*, but we give insertion, for the satisfaction of the writer and the information of his friends, to the following passage, which is strictly applicable to the case, from the excellent work of Jervis on the office and duty of Coroners. Nothing can be more evident than the fact that no inquest should have been held on the occasion in question:—

"The *dying suddenly* is not to be understood as relating to a fever, an apoplexy, or other visitation of God, and Coroners ought not in such cases, to obtrude themselves

into private families for the purpose of instituting inquiry, but should wait until they are sent for by the peace officers of the place, to whom it is the duty of those in whose houses *violent* or *unnatural* deaths occur, to make immediate communication, whilst the body is fresh, and, if possible, whilst it remains in the same situation as when the person died. But, under whatever circumstances, this authority must be exercised within the limits of a sound discretion; and unless there be *a reasonable ground of suspicion* that the party came to his death by *violent* and *unnatural* means, there is no occasion, except in the case of a person dying in jail, for the interference of the Coroner. In fact, Coroners have, on several occasions, been censured by the Court of King's Bench for holding repeated and unnecessary inquests, for the sake of enhancing their fees, where there was no reasonable probability that the deaths occurred from violence or unnatural causes."

A correspondent is anxious to throw out a suggestion, "That any new Board of Metropolitan University Examiners should have the power of conferring degrees in *surgery*, as at Glasgow and on the Continent, the candidates having bestowed on them, according to their acquirements, degrees of C.B., C.M., and C.D."

A Constant Reader.—If the party practise as an apothecary, he is liable to prosecution.

A Shareholder will see that the scheme of compulsory residence which his propositions involve, cannot obtain from us any support as a part of a code of regulations for conferring degrees in science.

We have not room this week for our reports from the Phrenological, the Medico-Chirurgical, and the Western Medical, Societies, and many communications from correspondents, though we have this week added eight pages to the usual Number of the Journal, are unavoidably omitted.

An account of the proceedings of the Medical Association of Tunbridge, which has been kindly forwarded to us by Mr. W. May, shall appear in the next Number of the THE LANCET.

Philo-Justitia, and the letter of our correspondent from Brighton, next week.

Mr. SCHLOSS has published a good lithographic sketch of the celebrated Professor TIEDEMANN. The likeness, taken while the Professor was lately in London, by a Mr. S. Laurence, is very faithful. It reminds us of the portraits of Abernethy, but the face has a more sedate and intellectual expression. A very cunning cast of countenance was given by most of the painters to the English surgeon.

The BRITISH MEDICAL ALMANACK for 1836 has just been published by Messrs. Sherwood. We have not room this week to say more on the subject of this annual, than that it is a work of infinite labour, and contains a vast fund of matter expressly interesting and useful to the members of the medical profession.

METEOROLOGICAL REPORT.

(*Extract from a Meteorological Journal kept at High Wycombe.*

Lat. 51° 37′ 44″ North. Long. 31° 45″ West.)

Days.	Thermometer.		Barometer.		Rain.	Wind.	Weather.
	Highest.	Lowest.	Highest.	Lowest.	Ins. Dcls.		
Nov. 16	42.25	34.50	29.78	29.73	—	N.W.	The weather during the week, generally dull and misty, with slight rain on the evening of the 22nd.—Almost brilliant Aurora borealis was visible for several hours on the night of the 18th.
17	46.25	40.50	29.68	29.63	—	W.	
18	48.75	35.25	.56	.46	—	W.	
19	46.25	32.50	.77	.72	—	W.	
20	49.50	42.	.75	.68	—	S.W.	
21	50.25	46.75	.66	.61	—	S.W.	
22	50.	47.25	.58	.49	0.21875	S.W.	
Nov. 23	48.25	39.50	29.58	29.05	—	S.	The early part of the week fine and warm for the season; afterwards rain daily, with intervals of clear and fine weather.
24	50.50	44.75	.58	.55	—	S.E.	
25	47.25	45.50	.54	.45	0.05	S.E.	
26	51.	48.50	.44	.14	0.24375	E.	
27	49.50	41.50	.14	.03	0.3375	S.E.	
28	45.50	34.50	.30	.07	0.0375	W.	
29	45.50	35.75	.29	.04	0.725	S.E.	

Dec. 1st, 1835.

W. JACKSON.